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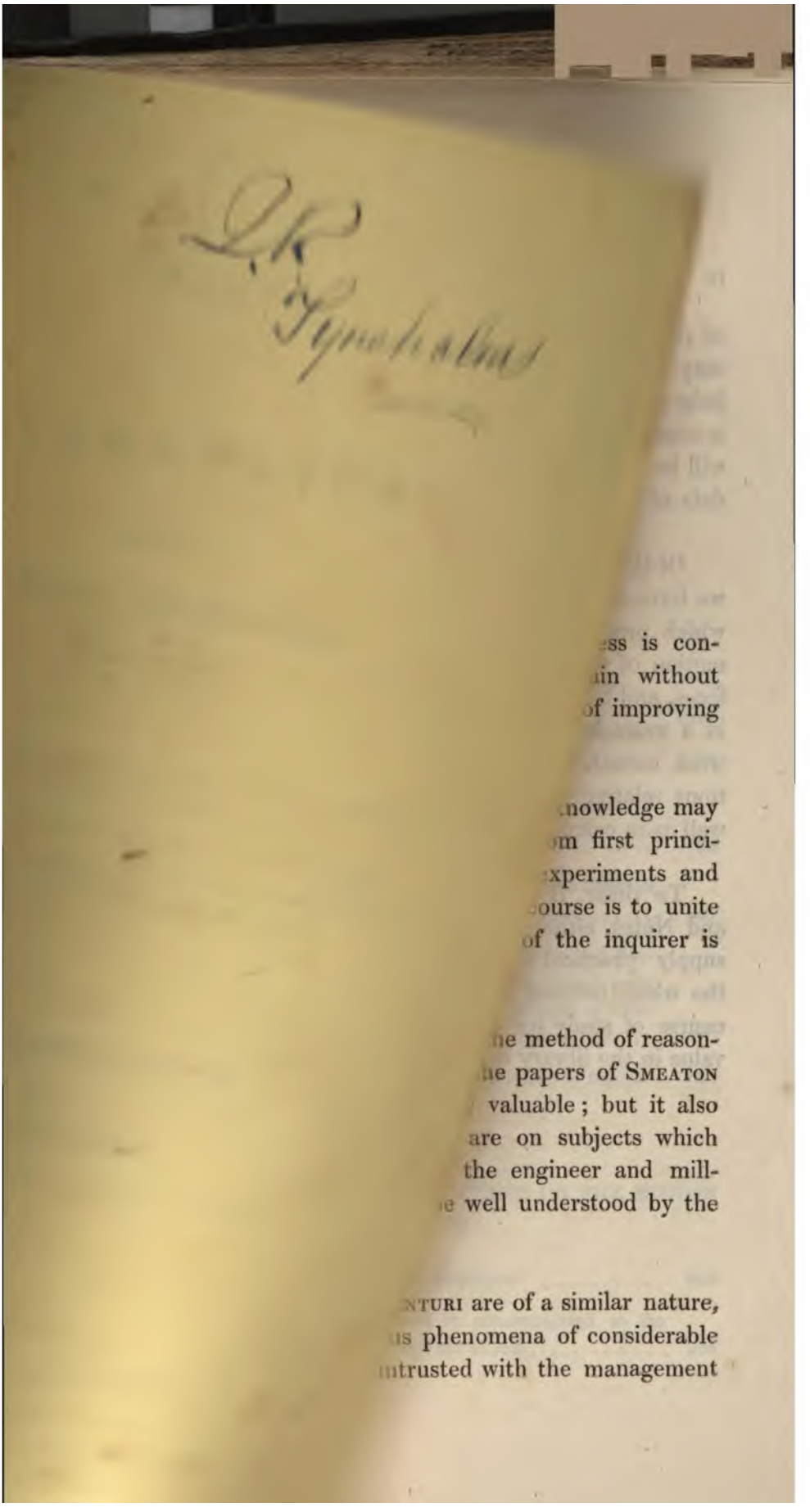

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LONDON:
PRINTED BY JAMES MOYES, CASTLE STREET,
LEICESTER SQUARE.

PREFACE.

It has been remarked, that when a business is conducted by precedent alone, it must remain without improvement; and that the best method of improving a business is to study it as a science.

Now, there are two modes by which knowledge may be acquired: the one by reasoning from first principles; the other by reasoning from experiments and observations of facts. But the best course is to unite the two methods, when the talent of the inquirer is equal to the task.

If it were only as examples of the method of reasoning from experiments and facts, the papers of Smeaton in their complete state are truly valuable; but it also fortunately happens that they are on subjects which form part of the business of the engineer and millwright, and which ought to be well understood by the Civil Engineer.

The experiments of Venturi are of a similar nature, and illustrate some curious phenomena of considerable importance; and those intrusted with the management

of rivers, drainage, and the supply of water by pipes, may study his experiments with much advantage. In judgment he seems as much inferior to Smeaton, as he is superior to him in mathematical learning. Hence it will be found that he sometimes builds too much on the data of his experiments.

Of the value of Dr. YOUNG's Summary of Hydraulics we have already given our opinion in the advertisement which precedes it; and we have only to add that the motion of water in rivers, the inclination of their surfaces, and the velocity of water in pipes, are there given in a more brief and clear manner than in any other work extant, and may be usefully referred to in questions relative to right of water in arbitrations, &c., as well as in calculations for new water-works.

To these, BOSSUT's Experiments on Water Wheels, and Notes, either to illustrate theoretical principles or supply practical rules, where wanting, are added; the whole forming a collection which will be a useful course of study for a young man, and not without its value in the library of the regular professor.

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AN EXPERIMENTAL INQUIRY

CONCERNING THE

NATURAL POWERS

OF

WATER AND WIND

TO TURN

MILLS AND OTHER MACHINES

DEPENDING ON A

CIRCULAR MOTION.

By Mr. JOHN SMEATON, F.R.S.

AN

EXPERIMENTAL INQUIRY

CONCERNING THE

NATURAL POWERS

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WATER AND WIND

TO TURN MILLS AND OTHER MACHINES DEPENDING ON A CIRCULAR MOTION.

Read before the Royal Society, May 3 *and* 10, 1759.

WHAT I have to communicate on this subject was originally deduced from experiments made on working models, which I look upon as the best means of obtaining the outlines in mechanical inquiries. But in this case it is very necessary to distinguish the circumstances in which a model differs from a machine in large; otherwise a model is more apt to lead us from the truth than towards it. Hence the common observation, that a thing may do very well in a model that will not answer in large. And, indeed, though the utmost circumspection be used in this way, the best structure of machines cannot be fully ascertained, but by making trials with them, when made of their proper size. It is for this reason that

though the models referred to, and the greater part of the following experiments, were made in the years 1752 and 1753, yet I deferred offering them to the Society, until I had an opportunity of putting the deductions made therefrom in real practice, in a variety of cases, and for various purposes; so as to be able to assure the Society that I have found them to answer.

PART I.

CONCERNING UNDERSHOT WATER WHEELS.

PLATE I. Fig. 1. is a perspective view of the machine for experiments on water-wheels; wherein

A B C D is the lower cistern or magazine, for receiving the water, after it has quitted the wheel; and for supplying

D E the upper cistern or head; wherein the water being raised to any height required by a pump, that height is shewn by

F G, a small rod, divided into inches and parts; with a float at the bottom, to move the rod up and down, as the surface of the water rises and falls.

H I is a rod by which the sluice is drawn, and stopped at any height required, by means of

K a pin, or peg, which fits several holes, placed in the manner of a diagonal scale, upon the face of the rod H I.

G L is the upper part of the rod of the pump, for drawing the

water out of the lower cistern, in order to raise and keep up the surface thereof at its desired height, in the head D E; thereby to supply the water expended by the aperture of the sluice.

M M is the arch and handle for working the pump, which is limited in its stroke by

N, a piece for stopping the handle from raising the piston too high; that also being prevented from going too low, by meeting the bottom of the barrel.

O is the cylinder, upon which a cord winds, and which, being conducted over the pullies P and Q, raises

R, the scale, into which the weights are put, for trying the power of the water.

S T, the two standards, which support the wheel, are made to slide up and down, in order to adjust the wheel, as near as possible, to the floor of the conduit.

W, the beam which supports the scale and pullies; this is represented as but little higher than the machine, for the sake of bringing the figure into a moderate compass, but, in reality, is placed 15 or 16 feet higher than the wheel.

PLATE II. Fig. 2. is a section of the same machine, wherein the same parts are marked with the same letters as in Fig. 1. Besides which,

X X is the pump barrel, being 5 inches diameter, and 11 inches long.

Y is the piston; and

Z the fixed valve.

G V is a cylinder of wood, fixed upon the pump-rod, and reaches above the surface of the water: this piece of wood being of such a thickness, that its section is half the area of that of the pump-barrel, will cause the surface of water to rise in the head, as much while the piston is descending, as while it is rising; and will thereby keep the gauge-rod F G more equally to its height.—*Note,* The arch and handle M M is here represented on a different side to what it is shewn in the preceding figures, in order that its dimensions may the better appear.

a a shews one of the two wires which serve as directors to the float, in order that the gauge-rod F G may be kept perpendicular; for the same purpose also serves *w*, a piece of wood with a hole to receive the gauge-rod, and keep it upright.

b is the aperture of the sluice.

c c a kant-board, for throwing the water more directly down the opening *c d,* into the lower cistern; and

c e is a sloping-board, for bringing back the water that is thrown up by the floats of the wheel.

Fig. 3. represents one end of the main axis: with a section of the movable cylinder, marked O in the preceding figures.

A B C D is the end of the axis; whereof the parts

B and D are covered with ferrules or hoops of brass.

E is a cylinder of metal; whereof the part marked

F is the pivot or gudgeon.

c c is the section of a hollow cylinder of wood, the diameter of the interior part being somewhat larger than the cylindrical ferrule B.

a a is the section of a ferrule of brass, driven into the end of the hollow cylinder, and which is adjusted to that marked B, so as to slide freely thereupon, but with as little shake as possible.

b b, *d d*, *g g*, represent the section of a brass ferrule, plate, and socket, fixed upon the other end of the hollow cylinder; the socket, *d d*, being adjusted to slide freely upon the cylinder E, in the same manner as the ferrule, *a a*, slides upon the cylinder B: the outer end of the socket at

g g is formed into a sort of button; by pushing whereof, the hollow cylinder will move backwards and forwards, or turn round at pleasure upon the cylindrical parts of the axis B and E.

e e, *i i*, *o o*, represent the section of a brass ferrule, also fixed upon the hollow cylinder: the edge of this ferrule

e e is cut into teeth, in the manner of a *contrate* wheel; and the edge thereof

o o is cut in the manner of a ratchet.

Of consequence, when the plate, *b d d b*, is pushed close to the ferrule D, the teeth of the ferrule *e e* will lay hold of

G, a pin fixed into the axis; by which means the hollow cylinder is made to turn along with the wheel and axis: but being drawn back by the button *g g*, the hollow cylinder is thereby disengaged from the pin G, and ceases

turning. *Note*, The weight in the scale is prevented from running back by a catch, that plays in and lays hold of the ratchet, *o o*.

By this means the hollow cylinder upon which the cord winds and raises the weight, is put in action and discharged therefrom instantaneously, while the wheel is in motion; for, without some contrivance of this kind, it would not be easy to make this sort of experiment with any tolerable degree of exactness.

The use of the apparatus now described will be rendered more intelligible, by giving a general idea of what I had in view; but as I shall be obliged to make use of a term which has heretofore been the cause of disputation, I think it necessary to assign the sense in which I would be understood to use it; and in which, I apprehend, it is used by practical *Mechanics*.

The word *Power*, as used in practical mechanics, I apprehend to signify the exertion of strength, gravitation, impulse, or pressure, so as to produce motion: and, by means of strength, gravitation, impulse, or pressure, compounded with motion, to be capable of producing an effect; and that no effect is properly mechanical, but what requires such a kind of power to produce it.

The raising of a weight, relative to the height to which it can be raised in a given time, is the most proper measure of power; or, in other words, if the weight raised is multiplied by the height to which it can be raised in a given time, the product is the measure of the power raising it; and, consequently, all those powers are equal, whose products, made by such multiplication, are equal: for if a power can raise twice the weight to the same height, or the same weight to twice the height, in the same time that another power can, the first power is double the second: and if a power can raise half the weight to double the

height, or double the weight to half the height, in the same time that another can, those two powers are equal. But *note*, all this is to be understood in case of slow or equable motion of the body raised; for in quick, accelerated, or retarded motions, the *vis inertiæ* of the matter moved will make a variation.*

In comparing the effects produced by water-wheels, with the powers producing them; or, in other words, to know what part of the original power is necessarily lost in the application, we must previously know how much of the power is spent in overcoming the friction of the machinery, and the resistance of the air; also, what is the real velocity of the water at the instant that it strikes the wheel; and the real quantity of water expended in a given time.

From the velocity of the water, at the instant that it strikes the wheel, given, the height of head productive of such velocity can be deduced, from acknowledged and experimented principles of hydrostatics: so that, by multiplying the quantity, or weight of water, really expended in a given time, by the height of a head so obtained, which must be considered as the height from which that weight of water had descended in that given time, we shall have a product, equal to the original power of the water, and clear of all uncertainty that would arise from the friction of the water in passing small apertures: and from all doubts, arising from the different measures of spouting waters, assigned by different authors. On the other hand, the sum of the weights raised by the action of this water, and of the weight required to overcome the friction and resistance

* When the velocity of a moving force is uniform, the space described is as the velocity; therefore, the power is proportional either to the force multiplied by the velocity, or the force multiplied by the space described. Hence, the slowness or quickness of the motion is indifferent, provided it be uniform; and the word Power is used by the author to express a force acting with uniform intensity and uniform velocity.—Ed.

the velocities multiplied by the height to which the weight can be raised in the time given, the product will be equal to the effect of that power; and the proportion of the two products will be the proportion of the *power* to the *effect*; so that by loading the wheel with different weights successively, we shall be able to determine at what particular load, and velocity of the wheel, the effect is a maximum.

The manner of finding the real velocity of the water, at the instant of its striking the wheel; the manner of finding the value of the friction, resistance, &c. in any given case; and the manner of finding the real expense of water, so far as concerns the following experiments, without having recourse to theory; being matters upon which the following determinations depend, it will be necessary to explain them.

To determine the Velocity of the Water striking the Wheel.

It has already been mentioned, in the reference to the figures, that weights are raised by a cord winding round a cylindrical part of the axis. First, then, let the wheel be put in motion by the water, but without any weights in the scale; and let the number of turns in a minute be 60: now it is evident, that were the wheel free from friction and resistance, that 60 times the circumference of the wheel would be the space through which the water would have moved in a minute, with that velocity wherewith it struck the wheel; but the wheel being encumbered by friction and resistance, and yet moving 60 turns in a minute, it is plain that the velocity of the water must have been greater than the circumference before it met with the wheel. Let now the cord be wound round the cylinder, but contrary to the usual way, and put a weight in the scale; the weight so disposed (which may be called the *counter-weight*) will endeavour to retard the wheel in turning the same way as it would have been turned by the water: put, therefore, as much weight into the scale as, without any water, will cause it to turn somewhat

faster than at the rate of 60 turns in a minute; suppose 63; let it now be tried again by the water, assisted by the weight; the wheel, therefore, will now make more than 60 turns; suppose 64: hence we conclude the water still exerts some power in giving motion to the wheel. Let the weight be again increased so as to make 64½ turns in a minute without water: let it once more be tried with water as before; and suppose it now to make the same number of turns with water as without, viz. 64½: hence, it is evident, that in this case the wheel makes the same number of turns in a minute, as it would do if the wheel had no friction or resistance at all, because the weight is equivalent thereto; for, was it too little, the water would accelerate the wheel beyond the weight, and, if too great, retard it; so that the water now becomes a *regulator* of the wheel's motion; and the velocity of its circumference becomes a measure of the velocity of the water.

In like manner, in seeking the greatest product or *maximum* of effect, having found by trials what weight gives the greatest product, by simply multiplying the weight in the scale by the number of turns of the wheel, find what weight in the scale, when the cord is on the contrary side of the cylinder, will cause the wheel to make the same number of turns the same way, without water. It is evident that this weight will be nearly equal to all friction and resistance taken together; and, consequently, that the weight *in* the scale, with twice* the weight *of* the scale, added to the back or counter-weight, will be equal to the weight that could have been raised, supposing the machine had been without friction or resistance; and which multiplied by the height to which it was raised, the product will be the greatest effect of that power.†

* The weight of the scale makes part of the weight both ways.

† This method of finding the friction does not give the actual resistance when the maximum power and load was upon the wheel, but only the friction from the

The Quantity of Water expended is found thus:—

The pump made use of for replenishing the head with water was so carefully made, that, no water escaping back by the leathers, it delivered the same quantity of water at every stroke, whether worked quick or slow; and as the length of the stroke was limited, consequently, the value of one stroke (or, on account of more exactness, 12 strokes) was known by the height to which the water was thereby raised in the head; which, being of a regular figure, was easily measured. The sluice, by which the water was drawn upon the wheel, was made to stop at certain heights by a peg; so that when the peg was in the same hole, the aperture for the effluent water was the same. Hence the quantity of water expended by any given head, and opening of the sluice, may be obtained: for, by observing how many strokes a minute was sufficient to keep up the surface of the water at the given height, and multiplying the number of strokes by the value of each, the water expended by any given aperture and head in a given time will be given.

These things will be further illustrated by going over the particulars of one set of experiments.

Specimen of a Set of Experiments.

The sluice drawn to the first hole.

The water above the floor of the sluice..30 inches.
Strokes of the pump in a minute 39½
The head raised by twelve strokes.....21

The wheel raised the empty scale, and made turns in a min. 80
With a counter weight of 1lb. 8 oz. it made..... 85
Laden, and load with water 86

Weight of the apparatus when moving at the velocity corresponding to the maximum. Consequently, the effect is every where estimated at less than its true value: this unnecessary circumstance cannot now be corrected.—ED.

No.	Weight. lb.	oz.	Tuns in a Min.	Product.	
1	4	0	45	180	
2	5	0	42	210	
3	6	0	36¼	217½	
4	7	0	33¾	236¼	
5	8	0	30	240	maximum.
6	9	0	26½	238½	
7	10	0	22	220	
8	11	0	16½	181½	
9	12	*	ceased working.		

Counter-weight, for 30 turns without water, 2 oz. in the scale.

N.B. The area of the head was 105·8 square inches.
Weight of the empty scale and pulley, 10 oz.
Circumference of the cylinder, 9 inches.
Circumference of the water-wheel, 75 ditto.

Reduction of the above Set of Experiments.

The circumference of the wheel, 75 inches, multiplied by 86 turns, gives 6450 inches for the velocity of the water in a minute; $\frac{1}{60}$ of which will be the velocity in a second, equal to 107·5 inches, or 8·96 feet, which is due to a head of 15 inches†; and this we call the *virtual* or *effective* head.‡

* N. B. When the wheel moves so slow as not to rid the water so fast as supplied by the sluice, the accumulated water falls back upon the aperture, and the wheel immediately ceases moving.

† This is determined upon the common maxim of hydrostatics, that the velocity of spouting waters is equal to the velocity that a heavy body would acquire in falling from the height of the reservoir; and is proved by the rising of jets to the height of their reservoirs nearly.

‡ The effective head differs from the real head by the velocity lost by the fluid in issuing through the sluice aperture, and the friction of the channel; and the

The area of the head being 105·8 inches, this multiplied by the weight of water of the inch cubic, equal to the decimal ·579 of the ounce avoirdupoise, gives 61·26 ounces for the weight of as much water as is contained in the head, upon 1 inch in depth, $\frac{1}{16}$ of which is 3·83 pounds: this multiplied by the depth 21 inches, gives 80·43 lbs. for the value of 12 strokes; and, by proportion, 39½ (the number made in a minute) will give 264·7 lbs. the weight of water expended in a minute.

Now, as 264·7 lbs. of water may be considered as having descended through a space of 15 inches in a minute, the product of these two numbers, 3970, will express the *power* of the water to produce mechanical effects; which were as follow:

The velocity of the wheel at the *maximum*, as appears above, was 30 turns in a minute; which, multiplied by 9 inches, the circumference of the cylinder, makes 270 inches: but, as the scale was hung by a pulley and double line, the weight was only raised half of this, viz. 135 inches.

	lbs.	oz.
The weight in the scale at the maximum	8	0
Weight of the scale and pulley	0	10
Counter-weight, scale, and pulley.....	0	12
Sum of the resistances.....	9	6
or *lbs.*	9·375.	

Now, as 9·375 lbs. are raised 135 inches, these two numbers being multiplied together, the product is 1266, which expresses the effect produced at a maximum: so that the proportion of the *power* to the *effect* is as 3970 : 1266, or as 10 : 3·18.

comparison of the effective and real heads ought to afford us the measure of the discharge from such an aperture, were it not that the motion of the pump appears to have affected the results.—Ed.

But though this is the greatest *single* effect producible from the power mentioned, by the impulse of the water upon an undershot wheel; yet, as the whole power of the water is not exhausted thereby, this will not be the true ratio between the *power* of the water and the *sum* of all the *effects* producible therefrom: for as the water must necessarily leave the wheel with a velocity equal to the wheel's circumference, it is plain that some part of the power of the water must remain after quitting the wheel.

The velocity of the wheel at the maximum is 30 turns in a minute: and, consequently, its circumference moves at the rate of 3·123 feet a second, which answers to a head 1·82 inches; this being multiplied by the expense of water in a minute, viz. 264·7 lbs. produces 481 for the power *remaining* in the water after it has passed the wheel: this being, therefore, deducted from the original power 3970, leaves 3489, which is that *part* of the power which is spent in producing the effect 1266; and, consequently, the part of the power spent in producing the effect, is to the greatest effect producible thereby, as 3489 : 1266 : : 10 : 3·62, or as 11 to 4.

The *velocity of the water* striking the wheel has been determined to be equal to 86 circumferences of the wheel per minute, and the *velocity of the wheel* at the *maximum* to be 30; the velocity of the water will, therefore, be to that of the wheel as 86 to 30: or as 10 to 3·5, or as 20 to 7.

The *load at the maximum* has been shewn to be equal to 9 lbs. 6 oz. and that the wheel ceased moving with 12 lbs. in the scale; to which if the weight of the scale is added, viz. 10 ounces,* the proportion will be nearly as 3 to 4 between

* The resistance of the air in this case ceases, and the friction is not added, as 12 lbs. in the scale were sufficient to stop the wheel after it had been in full motion; and, therefore, somewhat more than a counterbalance to the impulse of the water.

the load at the *maximum*, and that by which the wheel is stopped.

It is somewhat remarkable, that though the velocity of the wheel, in relation to the water, turns out greater than $\frac{1}{3}$ of the velocity of the water, yet the impulse of the water in the case of a *maximum* is more than double of what is assigned by theory; that is, instead of $\frac{4}{9}$ of the column, it is nearly equal to the whole column.*

* A brief analysis of the theory of undershot wheels will, perhaps, set some of these questions at rest, though conducted by an indirect process.

The power of the water is equal to the velocity and pressure due to the effective head.

The resistance, reduced to the circumference of the wheel, is equal to the weight raised, added to the friction of the machine when raising that weight, multiplied by the velocity of the wheel.

The maximum of useful effect takes place when the greatest weight is raised with the greatest velocity.

Let P be the pressure due to the effective head, and V the velocity due to the same head; and v the velocity of the wheel.

Also, suppose the velocity with which the water quits the wheel, to be n times the velocity of the wheel, or $n\,v$; for it may easily be proved, either by theory or experiment, that it must escape with a greater velocity than the wheel, if not confined to it for a definite portion of its revolution; while, it is self-evident, that it cannot escape with a less velocity than that of the wheel; and that, therefore, the greatest effect of the stream can be procured only when the water quits with the same velocity as the wheel. The determination of the portion of the revolution I do not enter upon here, but merely the maximum as dependent on the relation between the velocity of the wheel, and that of the free stream.

The effective pressures are as the squares of the velocities, in the uniform motions of fluids; hence,

$$V^2 : n^2v^2 :: P : \frac{n^2v^2\,P}{V^2} = \text{the force of the stream}$$

when it quits the wheel, hence

$$P - \frac{P\,n^2v^2}{V^2} = \text{the force communicated to the wheel;}$$

which being multiplied by the velocity of the wheel, and the co-efficient a, as an approximate measure of the increase due to the slow motion of the wheel, we have

$$a\,P\,(v - \frac{n^2v^3}{V^2}) = \text{the power of the water on the wheel.}$$

It must be remembered, therefore, that in the present case, the wheel was not placed in an open river, where the natural current, after it has communicated its impulse to the float, has

If R be the working resistance, and fR the friction, both reduced to their effect at the circumference of the wheel, then, neglecting the friction due to the weight of the machine, $(R+fR)v=Rv(1+f)=$ the total resistance, of which Rv is to be the greatest possible; therefore,

$$\frac{Pa}{1+f}\left(v-\frac{n^2v^3}{V^2}\right)=\text{ a maximum; or,}$$

$V^2v-n^2v^3=$ a maximum. Whence its fluxion $V^2\dot{v}-3n^2v^2\dot{v}=o$, and, consequently, $V^2=3n^2v^2$, or $vn=\frac{V}{\sqrt{3}}=\cdot5774V$; and $v=\frac{\cdot5774V}{n}$.

The effect of an undershot water-wheel is therefore a maximum when the stream escapes with 0·5774 times the velocity with which the water first strikes the wheel.

Again, the effective or useful pressure communicated to the wheel is $\frac{Pa}{1+f}\left(\frac{V^2-n^2v^2}{V^2}\right)$; and substituting for v^2 its value $\frac{V^2}{3n^2}$ it becomes $\frac{Pa}{1+f}(1-\frac{1}{3})=\frac{2Pa}{3(1+f)}$. And when not reduced for friction, $\frac{2}{3}Pa$

The resistance at the maximum is to the resistance that would stop the wheel as $2a:3$. Hence, by theory, the force is greater than two-thirds of the column, and the velocity greater than one-third of that of the water.

Also, if the friction were a constant fraction of the pressure, the load at the maximum would be to the load that stops the wheel as $2a:3$. But the friction cannot be a constant fraction in the same machine, because it depends on the weight of the wheel and apparatus, and the pressure and resistance; the one part of which is invariable and the other variable.

Having found the velocity of the wheel to be $\frac{\cdot5774V}{n}$, and the pressure $\frac{2Pa}{3}$ the whole effect will be $\frac{2\times\cdot5774PVa}{3n}=\frac{\cdot3849PVa}{n}$. That is, the ratio of the power of the water is to the whole effect, including friction, as 10 : 3·849, when the water is confined to the wheel so as to escape at the same velocity.

If we allow one-sixth of the stress to be expended in friction and bending the cord, the least certainly that would take place in the apparatus, the ratio of the power to the effect, as given in the table, will be as 10 : 3·2, the constant part of the friction being there allowed for.

room on all sides to escape, as the theory supposes; but in a conduit, or race, to which the float being adapted, the water cannot otherwise escape than by moving along with the wheel. It is observable, that a wheel working in this manner, as soon as the water meets the float, receiving a sudden check, it rises up against the float, like a wave against a fixed object; insomuch that when the sheet of water is not a quarter of an inch thick before it meets the float, yet this sheet will act upon the whole surface of a float, whose height is three inches; and consequently was the float no higher than the thickness of the sheet of water, as the theory also supposes, a great part of the force would have been lost, by the water dashing over the float.*

In further confirmation of what is already delivered, I have adjoined a table (TABLE I), containing the result of 27 sets

This result does not differ more from the experiments than they differ from each other.

The theory alluded to in the text, is founded on the supposition that the water simply strikes against the floats, and does not follow them; but the supposition is not true, and therefore the calculations do not agree with experiment.

Another mode of calculation was proposed by Don George Juan, and followed by M. Prony, but its non-accordance with some of the results of observation rendered it necessary to examine the question anew.—ED.

* Since the above was written, I find that Professor Euler, in the Berlin Acts for the year 1748, in a memoir entitled *Maxims pour arranger le plus avantageusement les Machines destinees à elever de l'Eau par le moyen de Pompes*, page 192, § 9, has the following passage; which seems to be the more remarkable, as I do not find he has given any demonstration of the principle therein contained, either from theory or experiment; or has made any use thereof in his calculations on this subject: —" Cependant dans ce cas puisque l'eau est reflechie, et qu'elle decoule sur les aubes vers, les cotés, elle y exerce encore une force particulière, dont l'effet de l'impulsion sera augmenté; et expérience jointe à la théorie a fait voir que dans ce cas, la force est presque double; de sorte qu'il faut prendre le double de la section du fil d'eau pour ce qui repond dans ce cas à la surface des aubes, poûrvu qu'elles soient assez larges pour recevoir ce supplement de force. Car si les aubes n'étoient plus larges que le fil, on trait d'eau on ne devroit prendre qu'une simple section, tout comme dans le premier cas, on l'aube toute entière est pappée par l'eau."

of experiments, made and reduced in the manner above specified. What remains of the theory of undershot wheels, will naturally follow from a comparison of the different experiments together.

TABLE I.

No.	Height of water in the cistern.	Turns of the wheel unloaded.	Virtual head deduced therefrom.	Turns at the maximum.	Load at the equilibrium.	Load at the maximum.	Water expended in a minute.	POWER.	EFFECT.	Ratio of the power and effect.	Ratio of the velocity of the water and wheel.	Ratio of the load at the equilibrium, to the load at the maximum.	Experiments.
	In.		In.		lb. oz.	lb. oz.							
1	33	88	15·85	30·	13 10	10 9	275·	4358	1411	10:3·24	10:3·4	10:7·75	
2	30	86	15·0	30·	12 10	9 6	264·7	3970	1266	10:3·2	10:3·5	10:7·4	
3	27	82	13·7	28·	11 2	8 6	243·	3329	1044	10:3·15	10:3·4	10:7·5	
4	24	78	12·3	27·7	9 10	7 5	235·	2890	901·4	10:3·12	10:3·55	10:7·53	At
5	21	75	11·4	25·9	8 10	6 5	214·	2439	735·7	10:3·02	10:3·45	10:7·32	the
6	18	70	9·95	23·5	6 10	5 5	199·	1970	561·8	10:2·85	10:3·36	10:8·02	1st
7	15	65	8·54	23·4	5 2	4 4	178·5	1524	442·5	10:2·9	10:3·6	10:8·3	hole.
8	12	60	7·29	22·	3 10	3 5	161·	1173	328·	10:2·8	10:3·77	10:9·1	
9	9	52	5·47	19·	2 12	2 8	134·	733	213·7	10:2·9	10:3·65	10:9·1	
10	6	42	3·55	16·	1 12	1 10	114·	404·7	117	10:2·82	10:3·8	10:9·3	
11	24	84	14·2	30·75	13 10	10 14	342·	4890	1505	10:3·075	10:3·66	10:7·9	
12	21	81	13·5	29·	11 10	9 6	297·	4009	1223	10:3·01	10:3·62	10:8·05	
13	18	72	10·5	26·	9 10	8 7	285·	2993	975	10:3·25	10:3·6	10:8·75	At
14	15	69	9·6	25·	7 10	6 14	277·	2659	774	10:2·92	10:3·62	10:9·	the
15	12	63	8·0	25·	5 10	4 14	234·	1872	549	10:2·94	10:3·97	10:8·7	2d.
16	9	56	6·37	23·	4 0	3 13	201·	1280	390	10:3·05	10:4·1	10:9·5	
17	6	46	4·25	21·	2 8	2 4	167·5	712	212	10:2·98	10:4·55	10:9·	
18	15	72	10·5	29·	11 10	9 6	357·	3743	1210	10:3·23	10:4·02	10:8·05	
19	12	66	8·75	26·75	8 10	7 6	339·	2887	878	10:3·05	10:4·05	10:8·1	The
20	9	58	6·8	24·5	5 8	5 0	255·	1734	541	10:3·01	10:4·22	10:9·1	3d.
21	6	48	4·7	23·5	3 2	3 0	228·	1064	317	10:2·99	10:4·9	10:9·6	
22	12	68	9·3	27·	9 2	8 6	359·	3338	1006	10:3·02	10:3·97	10:9·17	
23	9	58	6·8	26·25	6 2	5 13	332·	2257	686	10:3·04	10:4·52	10:9·5	4th.
24	6	48	4·7	24·5	3 12	3 8	262·	1231	385	10:3·13	10:5·1	10:9·35	
25	9	60	7·29	27·3	6 12	6 6	355·	2588	785	10:3·03	10:4·55	10:9·45	5th.
26	6	50	5·03	24·6	4 6	4 1	307·	1544	450	10:2·92	10:4·9	10:9·3	
27	6	50	5·03	26·	4 15	4 9	360·	1811	534	10:2·95	10:5·2	10:9·25	6th.
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	

MAXIMS AND OBSERVATIONS DEDUCED FROM THE FOREGOING TABLE OF EXPERIMENTS.

Maxim I. *That the virtual or effective head being the same, the effect will be nearly as the quantity of water expended.**

This will appear by comparing the contents of the columns 4, 8, and 10, in the foregoing sets of experiments; as for

Example 1st, *taken from* No. 8 *and* 25. viz.

No.	Virtual Head.	Water expended.	Effect.
8	7·29	161	328
25	7·29	355	785

Now the heads being equal, if the effects are proportioned to the water expended, we shall have by maxim 1st, 161 : 355 :: 328 : 723; but 723 falls short of 785, as it turns out in experiment, according to No. 25, by 62; the effect therefore of No. 25, compared with No. 8, is greater than according to the present maxim in the ratio of 14 to 13.

* When P is the pressure of the effective head, and V the velocity, it is shewn in the note on page 14, that the effect at the maximum is ·3849 P V. Now when P is constant, the effect varies, as the velocity V; and the quantity of water flowing in a given time being as the velocity, therefore the effect must be as the quantity of water expended, all other circumstances being the same.— Ed.

The foregoing example, with four similar ones, are seen at one view in the following Table.

Examples.	No. Tab. I.	Virtual Head.	Expense of Water.	Effect.	Comparison.	Variation.	Proportional Variation.
		Inch.	♁				
1st	8	7·29	161	328	161 : 355 :: 328 : 723	62 +	14 : 13
	25	7·29	355	785			
2d	13	10·5	285	975	285 : 357 :: 975 : 1221	11 —	121 : 122
	18	10·5	357	1210			
3d	20	6·8	255	541	255 : 332 :: 541 : 704	18 —	38 : 39
	23	6·8	332	686			
4th	21	4·7	228	317	228 : 262 :: 317 : 364	21 +	18 : 17
	24	4·7	262	385			
5th	26	5·03	307	450	307 : 360 :: 450 : 531	3 +	178 : 177
	27	5·03	360	534			

Hence, therefore, in comparing different experiments, as some fall short, and others exceed the maximum, and all agree therewith, as near as can be expected, in an affair where so many different circumstances are concerned, we may, according to the laws of reasoning by induction, conclude the maxim true: viz., that the effects are nearly as the quantity of water expended.

Maxim II. *That the expense of water being the same, the effect will be nearly as the height of the virtual or effective head.**

This also will appear by comparing the contents of columns 4, 8, and 10, in any of the sets of experiments.

Example 1*st of* No 2, *and* No. 24, *viz.*

No.	Virt. Head.	Expense.	Effect.
2	15	264·7	1266
24	4·7	262	385

Now, as the expenses are not quite equal, we must proportion one of the effects accordingly: thus,

by maxim 1st..	262 : 264·7 ::	385 :	389
and by max. 2d	15 : 4·7 ::	1266 :	397
	Difference		8

The effect, therefore, of No. 24, compared with No, 2, is less than according to the present maxim in the ratio of 49 : 50.

* The pressure of the effective head is as the height of that head, and the area of the aperture, that is P : ha, where a is the area, and h the height of the effective head; hence PV : haV. That is, the effect is directly as the head of water, the area of the aperture and the velocity. But when the expense is to be the same, the area of the aperture multiplied by the velocity must be constant; whence the effect, in that case, is simply as the effective head.—Ed.

The foregoing, and two other similar examples, are comprised in the following Table:

No. Tab. I. / Examples.	Virtual Head.	Expense of Water.	Effect.	Comparison.	Variation.	Proportional Variation.
1st { 2	15	264·7	1266	Max. 1st, 262 : 264·7 :: 385 : 319	8 —	49 : 50
1st { 24	4·7	262	385	Max. 2nd, 15 : 4·7 :: 1266 : 397		
2d { 1	15·85	275	1411	Max. 1st, 114 : 275 :: 117 : 282	34 —	8 : 9
2d { 10	3·55	114	117	Max. 2d, 15·85 : 3·55 :: 1411 : 316		
3d { 11	14·2	342	1505	Max. 1st, 167·5 : 342 :: 212 : 433	17 —	25 : 26
3d { 17	4·25	167·5	212	Max. 2nd, 14·2 : 42·5 :: 1505 : 450		

Maxim III. *That the quantity of water expended being the same, the effect is nearly as the square of its velocity.**

This will appear by comparing the contents of columns 3, 8, and 10, in any of the sets of experiments; as, for

Example 1*st, of* No. 2, *with* No. 24, *viz.*

No.	Turns in a min.	Expense.	Effect.
2	86	264·7	1266
24	48	262	385

The velocity being as the number of turns, we shall have,

by max. 1st, $262 : 264{\cdot}7 :: 385 : 389$

and by max 3d, $\left.\begin{matrix} 86^2 : 48^2 \\ 7396 : 2304 \end{matrix}\right\} :: 1266 : 394$

Difference 5

The effect, therefore, of No. 24, compared with No. 2, is less than by the present maxim in the ratio of 78 : 79.

The foregoing, and three other similar examples, are comprised in the following Table:

* In the last note (p. 20) it is shewn that the expense of water being the same, the effect is as the effective head; but the square of the velocity of water flowing from an aperture also varies as the effective head, consequently the effect will vary as the square of the velocity when the expense of water is the same.—Ed.

Examples.	No. Table I.	Turns in a minute.	Expense of Water.	Effect.	Comparison.	Variation.	Proportional Variation.
1st	2	86	264·7	1266	Max. 1st, 262 : 264·7 :: 385 : 389	5 —	78 : 79
	24	48	262	385	Max. 3d, 86² : 48² :: 1266 : 394 7396 : 2304		
2d	1	88	275	1411	Max. 1st, 114 : 275 :: 117 : 282	39—	7 : 8
	10	42	114	117	Max. 3d, 88² : 42² :: 1411 : 321 7744 : 1764		
3d	11	84	342	1505	Max. 1st, 167·5 : 342 :: 212 : 433	18—	24 : 25
	17	46	167·5	212	Max. 3d, 84² : 46² :: 1505 : 451 7056 : 2116		
4th	18	72	357	1210	Max. 1st, 228 : 357 :: 317 : 496	42—	12 : 13
	21	48	228	317	Max. 3d, 72² : 48² :: 1210 : 538 5184 : 2301		

Maxim IV. *The aperture being the same, the effect will be nearly as the cube of the velocity of the water.**

This, also, will appear by comparing the contents of columns 3, 8, and 10; as, for

Example 1*st of* No. 1, *and* No. 10, *viz.*

No.	Turns.	Expense.	Effect.
1	88	275	1411
10	42	114	117

Lemma. It must here be observed, that if water passes out of an aperture, in the same section, but with different velocities, the expense will be proportional to the velocity; and, therefore, conversely, if the expense is not proportional to the velocity, the section of the water is not the same.

Now, comparing the water discharged with the turns of Nos. 1 and 10, we shall have 88 : 42 :: 275 : 131·2; but the water discharged by No. 10 is only 114lbs.; therefore, though the sluice were drawn to the same height in No. 10, as in No. 1, yet the section of the water passing out was less in No. 10 than No. 1, in the proportion of 114 to 131·2; consequently, had the effective aperture or section of the water been the same in No. 10 as in No. 1, so that 131·2lbs. of water had been discharged instead of 114, the effect would have been increased in the same proportion; that is,

by the *Lemma*,	88	:	42	::	275	: 131·2
by maxim 1st,	114	:	131·2	::	117	: 134·5
and by max. 4th,	83^3 / 681472	:	42^3 / 74088	::	1411	: 153·5
				Difference		19

* It has been shewn (in note, p. 20) that the effect is as the head of water multiplied by the area of the aperture, and by the velocity; and the square of the velocity being proportional to the head of water, the effect will vary as the area and cube of the velocity, or the aperture being constant as the cube of the velocity.—Ed.

The effect, therefore, of 10, compared with No. 1, is less than it ought to be by the present maxim in the ratio of 7 : 8.

The foregoing, and three other similar examples, are contained in the following Table.

Examples.	No. Table I.	Turns in a minute.	Expense of Water.	Effect.	Comparison.	Variation.	Proportional Variation.
1st	1	88	275	1411	Lemma, 88 : 42 :: 275 : 131·2	19—	7 : 8
	10	42	114	117	Max. 1, 114 : 131·2 :: 117 : 134·5		
					Max. 4, 88^3 : 42^3 :: 1411 : 153·5		
2d	11	84	342	1505	Lemma, 84 : 46 :: 342 : 187·3	10—	23 : 24
	17	46	167·5	212	Max. 1, 167·5 : 187·3 :: 212 : 237		
					Max. 4, 84^3 : 46^3 :: 1505 : 247		
3d	18	72	357	1210	Lemma, 72 : 48 :: 357 : 238	24—	14 : 15
	21	48	226	317	Max. 1, 228 : 238 :: 317 : 331		
					Max. 4, 72^3 : 48^3 :: 1210 : 355		
4th	22	68	359	1006	Lemma, 68 : 48 :: 359 : 253·4	18+	20 : 19
	24	48	262	385	Max. 1, 262 : 253·4 :: 385 : 372		
					Max. 4, 68^3 : 48^3 :: 1006 : 354		

OBSERVATIONS.

Observ. 1. On comparing column 2d and 4th, Tab. I. it is evident that the *virtual head* bears no certain proportion to the *head of water;* but that when the aperture is greater, or the velocity of the water issuing therefrom less, they approach nearer to a coincidence; and, consequently, in the large openings of mills and sluices, where great quantities of water are discharged from moderate heads, the head of water, and virtual head determined from the velocity, will nearly agree, as experience confirms.*

Observ. 2. Upon comparing the several proportions between the *power* and *effect* in column 11th, the most general is that of 10 to 3; the extremes, 10 to 3·2 and 10 to 2·8; but, as it is observable, that where the quantity of water, or the velocity thereof; that is, where the power is greatest, the 2d term of the ratio is greatest also: we may, therefore, well allow the proportion subsisting in large works, as 3 to 1.

Observ. 3. The proportions of *velocities* between the *water* and the *wheel* in column 12, are contained in the limits of 3 to 1 and 2 to 1; but, as the greater velocities approach the limit of 3 to 1, and the greater quantities of water approach to that of 2 to 1, the best general proportion will be that of 5 to 2.†

* It has been already remarked in a note on p. 11, that the motion of the water through the aperture must have been disturbed by the action of the pump, and which, of course, would not affect the result in practical cases. The actual allowance in practice for excess of the real head above the effective one must depend on the nature of the contractions that will take place before the water arrives at the wheel.—Ed.

† The ratio was contained between the limits of 3 to 1 and 1·92 to 1. In theory it may be contained between 3 to 1 and 1·732 to 1, and the later ratio will always have place when the water is retained to the wheel till it quits it with the wheel's velocity.—Ed.

OBSERV. 4. On comparing the numbers in column 13, it appears that there is no certain ratio between the *load* that the wheel will carry at its *maximum*, and what will totally stop it; but that they are contained within the limits of 20 to 19, and of 20 to 15; but, as the effect approaches nearest to the ratio of 20 to 15, or of 4 to 3, when the power is greatest, whether by increase of velocity, or quantity of water, this seems to be the most applicable to large works; but as the load that a wheel ought to have, in order to work to the best advantage, can be assigned, by knowing the effect it ought to produce, and the velocity it ought to have in producing it, the exact knowledge of the greatest load it will bear, is of the less consequence in practice.

It is to be noted, that in all the examples under the last three of the four preceding maxims, the effect of the lesser power falls short of its due proportion to the greater, when compared by its maxim; except the last example of maxim 4th: and hence, if the experiments are taken strictly, we must infer, that the effects increase and diminish in a higher ratio than those maxims suppose: but as the deviation is not very considerable, the greatest being about 1-8th of the quantity in question: and as it is not easy to make experiments of so compounded a nature with absolute precision, we may rather suppose, that the lesser power is attended with some friction, or works under some disadvantage, which has not been duly accounted for; and, therefore, we may conclude, that these maxims will hold very nearly when applied to works in large.

After the experiments above-mentioned were tried, the wheel, which had originally 24 floats, was reduced to twelve; which caused a diminution in the effect, on account of a greater quantity of water escaping between the floats and the floor; but a circular sweep being adapted thereto, of such a length, that one float entered the curve before the preceding

one quitted it, the effect came so near to the former, as not to give hopes of advancing it by increasing the number of floats beyond 24 in this particular wheel.*

THE FOLLOWING

EXPERIMENTS MADE BY L'ABBÉ BOSSUT ON UNDERSHOT WHEELS.

ARE ADDED BY THE EDITOR.

Bossut's Experiments on Undershot water-wheels are interesting, because they not only confirm the results, but also include circumstances not tried by Smeaton. Bossut tried the effect in a close canal, and in an open stream, and in both cases varied the number of floats.

The experiments in the close canal were made with a wheel, of which the extreme diameter was 3 feet $1\frac{5}{6}$ inches, 5 inches in breadth, and the depth of the floats, from 4 to 5 inches; the axis of the wheel which received the cord, 2 inches diameter, and the diameter of the gudgeons, $2\frac{1}{2}$ lines; the diameter of the pulley, $3\frac{2}{3}$ inches, and of its axis, $2\frac{2}{3}$ lines; and the diameter of the cord, 2 lines. The wheel was suspended freely on its axis, and within half a line of the bottom of the canal. The sluice was raised one inch, and the velocity of the current was 300 feet in 33 seconds.

The dimensions are in the French measures, and the weight the French pound.

* It seems that the construction indicated here, and practised by the author in his works, has been lately rewarded as a new discovery by the French Academy.—Ed.

Number of float-boards.	Weight raised in lbs.	Number of turns of the wheel in a minute.	Product of the turns by the weight raised.
48	12	33¼	399
48	16	28½	456
24	12	29	348
24	16	25½	408
12	12	25½	306
12	16	19¼	308
With the sluice raised 1 inch, velocity of the stream 300 feet in 30 seconds.			
48	12	42½	510
48	16	39	624
24	12	38	556
24	16	35½	568
12	12	31¼	375
12	16	28¾	345

From these experiments it appears that, *in a close canal*, the effect with 48 floats on a wheel of 3 feet 1⅝ inch diameter, is greater than that with a less number. The arc immersed was about a sixteenth part of the circumference of the wheel.

It had been observed, by Bossut, that the water-wheels used on rivers had a less number of float-boards; and the object of his next experiment was to determine whether the variation in practice would be justified by experiment or not. In the wheel used in these experiments the external diameter was 3 feet; the breadth, 6 inches; and the depth of the float boards, 6 inches. The diameter of the barrel round which the cord wound was 2½ inches; that of the gudgeons, ¼ of an inch. The pulley and cord the same as before. The whole weight of the moving parts of the apparatus was 44 lbs.

The experiments were made in an open current between 12 and 13 feet wide, and seven or eight inches deep, and the machine supported so as not to interfere with the motion of the stream. The float boards were immersed to the depth of four inches.

Number of floats.	Weight raised in pounds.	Number of turns of the wheel in a minute.	Product of the turns by the load.
48	24	$27\frac{19}{48}$	$657\frac{1}{2}$
24	24	$27\frac{7}{48}$	$651\frac{1}{2}$
24	40	$23\frac{3}{8}$	935
12	40	$19\frac{31}{48}$	779

It appears from these trials that the wheel had very little more effect with 48 boards than with 24, but that the difference between the effect of 24 and 12 is very considerable, the former being decidedly better: consequently less than 24, when the wheel dips two-ninths of its circumference, ought not to be used.

Bossut's next objects were to determine the relation between the velocity of the stream and that of the wheel in each of these cases, when a maximum of useful effect was obtained, using the same wheel for both cases as the last described trial was made with: he obtained the results given in the following tables.

EXPERIMENTS IN A CLOSE CANAL.

Sluice raised 2 inches, the velocity of stream 300 feet in 27 seconds, and 48 float boards to the wheel.

Weight raised in pounds.	Number of turns of the wheel in 40 seconds.	Product of the weight raised by the No. of turns.
$30\frac{1}{2}$	$22\frac{1}{4}$	$678\frac{1}{2}$
31	$22\frac{1}{2}$	$684\frac{1}{2}$
$31\frac{1}{2}$	$21\frac{7}{8}$	689
32	$21\frac{2}{3}$	693
$32\frac{1}{2}$	$21\frac{5}{12}$	696
33	$21\frac{1}{6}$	$698\frac{1}{2}$
$33\frac{1}{2}$	$20\frac{11}{12}$	$700\frac{3}{4}$
34	$20\frac{2}{3}$	703
$34\frac{1}{2}$	$20\frac{3}{16}$	$704\frac{1}{2}$ max.
35	$19\frac{11}{12}$	697
$35\frac{1}{2}$	$19\frac{5}{16}$	685
36	$18\frac{7}{12}$	669

The velocity of the stream is to the velocity of the exterior circumference of the wheel as 10 is to 4·32 when the power is a maximum. But the velocity of the centre of impression ought to be considered the velocity of the wheel, and then the ratio is as 10 : 4·1.

EXPERIMENTS IN AN OPEN STREAM.

Velocity of the stream 68½ inches per second; the float boards dipping 4 inches into the water, and the wheel had 24 float boards.

Weight raised in pounds.	Number of turns of the wheel in 40 seconds.	Product of the weight by the number of turns.
30	$17\frac{11}{24}$	$523\frac{3}{4}$
35	$16\frac{25}{48}$	$578\frac{1}{4}$
40	$15\frac{7}{12}$	$623\frac{1}{4}$
45	$14\frac{31}{48}$	659
50	$13\frac{17}{24}$	$685\frac{1}{2}$
55	$12\frac{37}{48}$	$702\frac{1}{2}$
56	$12\frac{7}{12}$	$704\frac{1}{2}$
57	$12\frac{19}{48}$	$706\frac{1}{2}$
58	$12\frac{5}{24}$	708
59	$12\frac{1}{48}$	$709\frac{1}{4}$
60	$11\frac{5}{6}$	710 max.
61	$11\frac{5}{8}$	709
62	$11\frac{19}{48}$	$706\frac{1}{2}$
63	$11\frac{7}{48}$	702
64	$10\frac{41}{48}$	695
65	$10\frac{25}{48}$	684
66	$10\frac{1}{16}$	664

The velocity of the stream is to the velocity of the exterior edges of the float-boards at the maximum as 10 is to 4·9 nearly, and to the velocity of the centre of impression as 10 is to 4·35. Hence it appears that the relation between the velocity of the stream and that of the wheel is about the same, whether it be in an open stream or in a confined channel.

The slow change in the effect, when the relation of the velocities is only slightly varied from the maximum, shews that we may in practice be satisfied with a near approximation to it.

It has generally been supposed that inclining the float-boards added to the power of the wheel; we shall, therefore, add the most accurate of Bossut's experiments on this point.

12 float-boards dipping into the stream 4 inches.	Weight raised in pounds.	Number of turns of the wheel in 40 seconds.	Product of the number of turns by the weight.
direct	40	$13\frac{17}{48}$	534
inclined 15°	40—	$14\frac{21}{48}$	$577\frac{1}{2}$
inclined 30°	40—	$14\frac{29}{48}$	$578\frac{1}{3}$
inclined 37°	40—	$14\frac{16}{48}$	$572\frac{1}{2}$

It appears from these trials that the greatest effect was obtained when the inclination of the floats to the radius was from 15° to 30°.

Bossut applied the term direct to the position when the plane of the float was directed to the centre of the wheel.—Ed.

PART II.

ON OVERSHOT WHEELS.

Read before the Royal Society, May 24, 1759.

In the former part of this essay, we have considered the impulse of a confined stream, acting on *Undershot Wheels*. We now proceed to examine the power and application of water, when acting by its *gravity* on *Overshot Wheels*.

In reasoning without experiment, one might be led to imagine, that however different the mode of application is, yet that whenever the same quantity of water descends through the same perpendicular space, that the natural effective power would be equal: supposing the machinery free from friction, equally calculated to receive the full effect of the power, and to make the most of it: for if we suppose the height of a column of water to be 30 inches, and resting upon a base or aperture of one square inch, every cubic inch of water that departs therefrom will acquire the same velocity, or *momentum*, from the uniform pressure of 30 cubic inches above it, that one cubic inch let fall from the top will acquire in falling down to the level of the aperture; viz. such a velocity as, in a contrary direction, would carry it to the level from whence

it fell;* one would therefore suppose, that a cubic inch of water let fall through a space of thirty inches, and there impinging upon another body, would be capable of producing an equal effect by collision, as if the same cubic inch had descended through the same space with a slower motion, and produced its effects gradually; for, in both cases, gravity acts upon an equal quantity of matter, through an equal space;† and, consequently, that whatever was the ratio between the power and effect in undershot wheels, the same would obtain in overshot, and, indeed, in all others; yet, however conclusive this reasoning may seem, it will appear in the course of the following deductions, that the effect of the gravity of descending bodies is very different from the effect of the stroke of such as are *non-elastic*, though generated by an equal mechanical power.

The alterations in the machinery already described, to accommodate the same for experiments on overshot wheels, were principally as follows:—

Plate II. *Fig.* 2. The sluice I *b* being shut down, the rod H I was unscrewed and taken off.

The undershot water-wheel was taken off the axis, and instead thereof, an overshot wheel of the same diameter was put into its place. *Note.* This wheel was two inches in the shroud, or depth of the bucket: the number of the buckets was 36.

* This is a consequence of the rising of jets to the height of their reservoirs nearly.

† Gravity, it is true, acts a longer space of time upon the body that descends slow than upon that which falls quick; but this cannot occasion the difference in the effect: for an elastic body falling through the same space in the same time, will, by collision upon another elastic body, rebound nearly to the height from which it fell; or by communicating its motion, cause an equal one to ascend to the same height.

The standards, S and T, *Fig.* 1, were raised half an inch, so that the bottom of the wheel might be clear of stagnant water.

A trunk, for bringing the water upon the wheel, was fixed according to the dotted lines *f g*, *Fig.* 2. The aperture was adjusted by a shuttle *h i*, which also closed up the outer end of the trunk, when the water was to be stopped.

Fig. 3. The ratchet *o o* not being of one piece of metal with the ferrule *e e*, *i i*, (though so described before, to prevent unnecessary distinctions) was, with its catch, turned the contrary side: consequently, the movable barrel would do its office equally, notwithstanding the water-wheel, when at work, moved the contrary way.

Specimen of a Set of Experiments.

Head 6 inches.

14½ strokes of the pump in a minute, 12 ditto = 80lb.*
Weight of the scale (being wet) 10½ oz.
Counterweight for 20 turns, besides the scale, 3 oz.

No.	Weight in the Scale.	Turns.	Product	Observations.
1	0lb.	60		Threw most part of the water out of the wheel.
2	1	56		
3	2	52		
4	3	49	147	Received the water more quietly
5	4	47	188	
6	5	45	225	
7	6	42½	255	
8	7	41	287	
9	8	38½	308	

* The small difference, in the value of 12 strokes of the pump, from the former experiments, was owing to a small difference in the length of the stroke, occasioned by the warping of the wood.

No.	Weight in the Scale.	Turns.	Product.	Observations.
10	9 lbs.	36½	328½	
11	10	35½	355	
12	11	32¾	360½	
13	12	31¼	375	
14	13	28½	370½	
15	14	27½	385	
16	15	26	390	
17	16	24½	392	
18	17	22¾	386¾	
19	18	21¾	391½	
20	19	20¾	394¼	maximum.
21	20	19¾	395	maximum.
22	21	18¼	388¼	
23	22	18	396	Worked irregular.
24	23	Overset by its load.		

Reduction of the preceding Specimen.

In these experiments the head being 6 inches, and the height of the wheel 24 inches, the whole descent will be 30 inches: the expense of water was 14½ strokes of the pump in a minute, whereof 12 contained 80 lbs.; therefore the water expended in a minute was 96⅔ lbs. which, multiplied by 30 inches, gives the *power* = 2900.

If we take the 20th experiment for the *maximum*, we shall have 20¾ turns in a minute, each of which raised the weight 4½ inches, that is, 93·37 inches in a minute. The weight *in* the scale was 19 lbs., the weight of the scale 10½ oz.; the counterweight 3 oz. in the scale, which, with the weight of the scale, 10½ oz., makes in the whole 20½ lbs., which is the whole resistance or load: this, multiplied by 93·37 inches, makes 1914 for the effect.

The *ratio*, therefore, of the *power* and *effect*, will be as 2900 : 1914, or as 10 : 6·6, or as 3 : 2 nearly.

But if we compute the power from the height of the wheel only, we shall have 96⅔ lbs. multiplied by 24 inches = 2320 for the *power*, and this will be to the *effect* as 2320 : 1914, or as 10 : 8·2, or as 5 : 4 nearly.

The reduction of this specimen is set down in No. 9 of the following table; and the rest were deduced from a similar set of experiments, reduced in the same manner.

TABLE II.

Containing the Result of Sixteen Sets of Experiments on Overshot Wheels.

No.	Whole descent.	Water expended in a minute.	Turns at the maximum, in a minute.	Weight raised at the maximum.	Power of the whole descent.	Power of the wheel.	Effect.	Ratio of the whole power and effect.	Ratio of power of the wheel and effect.	Mean Ratio.
	Inches.	*lbs.*		*lbs.*						
1	27	30	19	6½	810	720	556	10 : 6·9	10 : 7·7	
2	27	56⅔	16½	14½	1530	1360	1060	10 : 6·9	10 : 7·8	
3	27	56⅔	20¾	12½	1530	1360	1167	10 : 7·6	10 : 8·4	
4	27	63⅓	20½	13½	1710	1524	1245	10 : 7·3	10 : 8·2	
5	27	76⅔	21½	15½	2070	1840	1500	10 : 7·3	10 : 8·2	Medium 10:8·1
6	28½	73⅓	18¾	17½	2090	1764	1476	10 : 7·	10 : 8·4	
7	28½	96⅔	20¼	20½	2755	2320	1868	10 : 6·8	10 : 8·	10:8·2
8	30	90	20	19½	2700	2160	1755	10 : 6·5	10 : 8·1	
9	30	96⅔	20¾	20½	2900	2320	1914	10 : 6·6	10 : 8·2	
10	30	113⅓	21	23½	3400	2720	2221	10 : 6·5	10 : 8·2	10:8·2
11	33	56⅔	20¼	13½	1870	1360	1230	10 : 6·6	10 : 9·	
12	33	106⅔	22¼	21½	3520	2560	2153	10 : 6·1	10 : 8·4	
13	33	146⅔	23	27½	4840	3520	2846	10 : 5 9	10 : 8·1	10:8·5
14	35	65	19¾	16½	2275	1560	1466	10 : 6·5	10 : 9·4	
15	35	120	21½	25½	4200	2880	2467	10 : 5·9	10 : 8·6	
16	35	163½	25	26½	5728	3924	2981	10 : 5·2	10 : 7·6	10:8·5
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.

OBSERVATIONS AND DEDUCTIONS FROM THE FOREGOING EXPERIMENTS.

I.—*Concerning the Ratio between the Power and Effect of Overshot Wheels.*

The effective power of the water must be reckoned upon the whole descent; because it must be raised that height, in order to be in a condition of producing the same effect a second time.

The ratios between the *powers* so estimated, and the *effects* at the *maximum*, deduced from the several sets of experiments, are exhibited at one view in column 9. of Table II.; and from hence it appears, that those ratios differ from that of 10 to 7·6 to that of 10 : 5·2, that is, nearly from 4 : 3 to 4 : 2. In those experiments where the heads of water and quantities expended are least, the proportion is nearly as 4 : 3; but where the heads and quantities are greatest, it approaches nearer to that of 4 : 2; and, by a medium of the whole, the ratio is that of 3 : 2 nearly. We have seen before, in our observations upon the effects of undershot wheels, that the general ratio of the power to the effect, when greatest, was 3 : 1; *the effect, therefore, of overshot wheels, under the same circumstances of quantity and fall, is at a medium double to that of the undershot:* and, as a consequence thereof, *that non-elastic bodies, when acting by their impulse or collision, communicate only a part of their original power;* the other part being spent in changing their figure, in consequence of the stroke.

The powers of water, computed from the height of the wheel only, compared with the effects, as in column 10, appear to observe a more constant ratio: for, if we take the medium of each class, which is set down in column 11, we shall find the extremes to differ no more than from the ratio of 10 : 8·1 to that of 10 : 8·5; and as the second term of the ratio gradually increases from 8·1 to 8·5, by an increase of head from 3 inches to 11, the excess of 8·5 above 8·1 is to be imputed to the superior impulse

of the water at the head of 11 inches above that of 3 inches: so that if we reduce 8·1 to 8, on account of the impulse of the 3 inch head, *we shall have the ratio of the power, computed upon the height of the wheel only, to the effect at a maximum as* 10:8 *or as* 5 : 4 *nearly;* and from the equality of the ratio between power and effect, subsisting, where the constructions are similar, we must infer, *that the effects,* as well as the powers, *are as the quantities of water and perpendicular heights, multiplied together respectively.*

II.—*Concerning the most proper Height of the Wheel in proportion to the whole Descent.*

We have already seen, from the preceding observation, that the effect of the same quantity of water descending through the same perpendicular space, is double, when acting by its gravity upon an overshot wheel, to what the same produces when acting by its impulse upon an undershot. It also appears, that, by increasing the head from 3 inches to 11, that is, the whole descent, from 27 inches to 35, or in the ratio of 7 to 9 nearly, the effect is advanced no more than in the ratio of 8·1 to 8·4, that is, as 7 : 7·26; and, consequently, the increase of effect is not 1–7th of the increase of perpendicular height. Hence it follows, *that the higher the wheel is in proportion to the whole descent, the greater will be the effect;* because it depends less upon the impulse of the head, and more upon the gravity of the water in the buckets: and if we consider how obliquely the water issuing from the head must strike the buckets, we shall not be at a loss to account for the little advantage that arises from the impulse thereof: and shall immediately see of how little consequence this impulse is to the effect of an overshot wheel.

However, as every thing has its limits, so has this: for thus much is desirable, *that the water shall have somewhat*

greater velocity, than the circumference of the wheel, in coming thereon; otherwise the wheel will not only be retarded by the buckets striking the water, but thereby dashing a part of it over, so much of the power is lost.

The velocity that the circumference of the wheel ought to have, being known by the following deductions, the head requisite to give the water its proper velocity is easily computed from the common rules of hydrostatics; and will be found much less than what is generally practised.

III.—*Concerning the Velocity of the Circumference of the Wheel, in order to produce the greatest Effect.*

If a body is let fall freely from the surface of the head to the bottom of the descent, it will take a certain time in falling; and, in this case, the whole action of gravity is spent in giving the body a certain velocity: but if this body, in falling, is made to act upon some other body, so as to produce a mechanical effect, the falling body will be retarded; because a part of the action of gravity is then spent in producing the effect, and the remainder only giving motion to the falling body: and, therefore, *the slower a body descends, the greater will be the portion of the action of gravity applicable to the producing a mechanical effect;* and, in consequence, the greater that effect may be.

If a stream of water falls into the bucket of an overshot wheel, it is there retained until the wheel, by moving round, discharges it: of consequence the slower the wheel moves, the more water each bucket will receive; so that what is lost in speed, is gained by the pressure of a greater quantity of water acting in the buckets at once; and, if considered only in this light, the mechanical power of an overshot wheel to produce effects will be equal, whether it move quick or slow: but if we

attend to what has been just now observed of the falling body, it will appear that so much of the action of gravity as is employed in giving the wheel and water therein a greater velocity, must be subtracted from its pressure upon the buckets, so that, though the product made by multiplying the number of cubic inches of water acting in the wheel at once by its velocity will be the same in all cases; yet as each cubic inch, when the velocity is *greater*, does not press so much upon the bucket as when it is *less*, the power of the water to produce effects will be greater in the less velocity than in the greater: and hence we are led to this general rule, *that*, cæteris paribus, *the less the velocity of the wheel, the greater will be the effect thereof.* A confirmation of this doctrine, together with the limits it is subject to in practice, may be deduced from the foregoing specimen of a set of experiments.

From these experiments it appears that, when the wheel made about 20 turns in a minute, the effect was near upon the greatest. When it made 30 turns, the effect was diminished about $\frac{1}{20}$ part; but that when it made 40, it was diminished about $\frac{1}{4}$; when it made less than $18\frac{1}{4}$, its motion was irregular; and when it was loaded so as not to admit its making 18 turns, the wheel was overpowered by its load.

It is an advantage in practice, that the velocity of the wheel should not be diminished further than what will procure some solid advantage in point of power; because, *cæteris paribus*, as the motion is slower, the buckets must be made larger; and the wheel being more loaded with water, the stress upon every part of the work will be increased in proportion. *The best velocity for practice, therefore, will be such as when the wheel here used made about* 30 *turns in a minute; that is, when the velocity of the circumference is a little more than* 3 *feet in a second.*

Experience confirms that this velocity of 3 feet in a second is applicable to the highest overshot wheels, as well as the

lowest; and all other parts of the work being properly adapted thereto, will produce very nearly the greatest effect possible: however, this also is certain from experience, that *high wheels may deviate further from this rule, before they will lose their power, by a given aliquot part of the whole, than low ones can be admitted to do;* for a wheel of 24 feet high may move at the rate of six feet per second without losing any considerable part of its power;* and, on the other hand, I have seen a wheel of 33 feet high, that has moved very steadily and well with a velocity but little exceeding 2 feet.†

IV.—*Concerning the Load for an Overshot Wheel, in order that it may produce a Maximum.*

The maximum load for an overshot wheel is that which reduces the circumferences of the wheel to its proper velocity; and this will be known by dividing the effect it ought to produce in a given time by the space intended to be described by the circumference of the wheel in the same time: the

* The 24 feet wheel going at 6 feet in a second seems owing to the small proportion that the head (requisite to give the water the proper velocity of the wheel) bears to the whole height.

† In the whole of this reasoning it is evidently overlooked that the motion of a machine is one wherein the power and effect are equal at any uniform velocity. Hence, the advantage of slow motion at the wheel must be lost at the working point, and the true principles of adjusting the velocity must be to determine the conditions which give the least stress on the machine, and fewest moving parts, and, therefore, least friction, without losing effect by the quantity of fall necessary to give the water the same velocity as the wheel. There is another circumstance of much importance in practice; it is this—the water wheel which has a quick motion acts as a fly to regulate the motion of the machinery, whereas, if it move slowly, it requires regulation itself. This truth seems to have been ascertained by Smeaton at a later period; for, in 1796, we find him directing that an overshot wheel for an oil mill at Hull, of 27 feet diameter, should move at the rate of 6½ feet per second, and remarks that, with pace so slow as 3 feet per second, it might not move regularly and equally. (See Smeaton's Reports, Vol. II. p. 401. First edition.—Ed.)

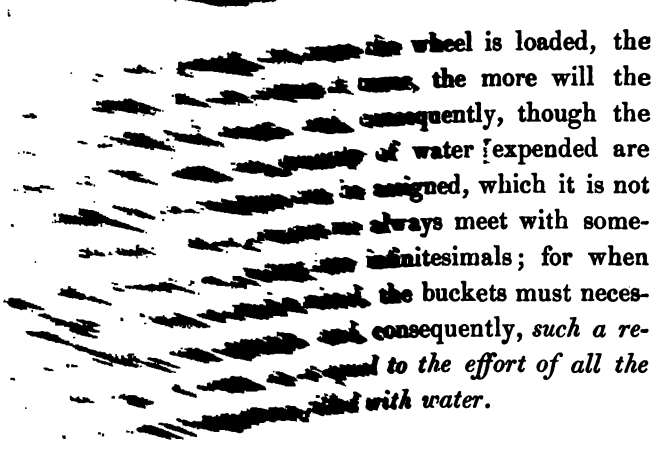
quotient will be the resistance overcome at the circumference of the wheel; and is equal to the load required, the friction and resistance of the machinery included.

V.—*Concerning the greatest possible Velocity of an Overshot Wheel.*

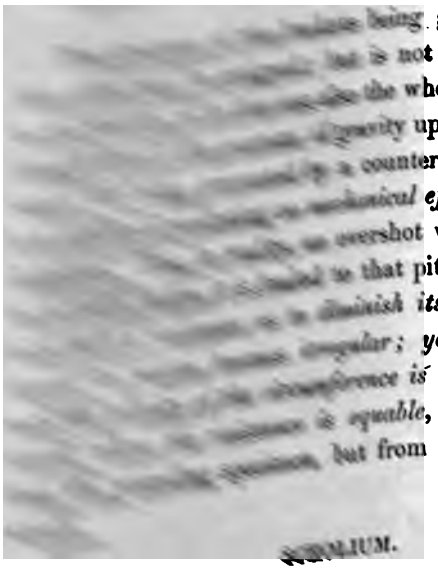
The greatest velocity that the circumference of an overshot wheel is capable of, depends jointly upon the diameter or *height* of the wheel, and the velocity of falling bodies; for it is plain that the velocity of the circumference can never be greater than to describe a semi-circumference, while a body let fall from the top of the wheel will descend through its diameter; nor, indeed, quite so great, as a body descending through the same perpendicular space cannot perform the same in so small a time when passing through a semicircle, as would be done in a perpendicular line. Thus, if a wheel is 16 feet 1 inch high, a body will fall through the diameter in one second: this wheel, therefore, can never arrive at a velocity equal to the making one turn in two seconds; but, in reality, an overshot wheel can never come near this velocity; for when it acquires a certain speed, the greatest part of the water is prevented from entering the buckets; and the rest, at a certain point of its descent, is thrown out again by the centrifugal force. This appears to have been the case in the three first experiments of the foregoing specimen; but, as the velocity, when this begins to happen, depends upon the form of the buckets, as well as other circumstances, *the utmost velocity of overshot wheels is not to be determined generally:* and, indeed, it is the less necessary in practice, as it is in this circumstance incapable of producing any *mechanical effect*, for reasons already given.

VI.—*Concerning the greatest Load that an Overshot Wheel can overcome.*

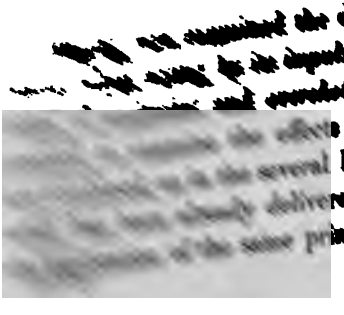
The greatest load an overshot wheel will overcome, considered abstractedly, is unlimited or infinite: for as the buckets may



wheel is loaded, the
the more will the
consequently, though the
of water expended are
signed, which it is not
always meet with some-
infinitesimals; for when
the buckets must neces-
consequently, *such a re-*
to the effort of all the
with water.



given, the quantity of
not of much consequence
the wheel loses its power; for
upon a given quantity of
counterbalance from moving,
effect, according to our
overshot wheel generally ceases
that pitch; for *when it meets*
its velocity to a certain
; yet this never happens
is less than two feet per
is equable, as appears not only
but from experiments on larger



different effects of the power of
and by its *weight*, under the
wheels; we might naturally
when the impulse and weight
kinds of *breast wheels*, &c.; but
being carefully attended to,
principles in these mixed cases will

be easy, and reduce what I have to say on this head into a narrow compass: for all kinds of wheels, where the water cannot descend through a given space, unless the wheel moves therewith, are to be considered of the nature of an overshot wheel, according to the perpendicular height that the water descends from; and all those that receive the impulse or shock of the water, whether in an horizontal, perpendicular, or oblique direction, are to be considered as undershots. And, therefore, a wheel, which the water strikes at a certain point below the surface of the head, and after that descends in the arc of a circle, pressing by its gravity upon the wheel; *the effect of such a wheel will be equal to the effect of an undershot, whose head is equal to the difference of level between the surface of the water in the reservoir and the point where it strikes the wheel, added to that of an overshot, whose height is equal to the difference of level between the point where it strikes the wheel and the level of the tail-water.* It is here supposed that the wheel receives the shock of the water at right angles to its radii; and that the velocity of its circumference is properly adapted to receive the utmost advantage of both these powers; otherwise a reduction must be made on that account.

Many obvious and considerable improvements upon the common practice naturally offer themselves, from a due consideration of the principles here established, as well as many popular errors shew themselves in view; but as my present purpose extends no further than the laying down such general rules as will be found to answer in practice, I leave the particular application to the intelligent artist, and to the curious in these matters.*

* Bossut's experiments on overshot water-wheels were made with a bucket-wheel, of which the exterior diameter was 3 feet; the breadth of the buckets, 5 inches; the depth of the bucket, or shrouding, 3 inches; and there were 48 buckets. The barrel round which the cord wound was $2\frac{7}{12}$ inches in diameter; and the gudgeons, $2\frac{1}{2}$ lines. The canal which brought the water to the wheel was horizontal, and of the same breadth as the wheel, and it delivered 1194 cubic inches per minute at the top of the wheel.

Weight raised in pounds.	Number of turns of the wheel in a minute.	Product of the turns by the weight raised.
unloaded.	$40\frac{1}{4}$	0
11	$11\frac{46}{48}$	$131\frac{1}{2}$
12	$11\frac{11}{48}$	$134\frac{3}{4}$
13	$10\frac{25}{48}$	$136\frac{3}{4}$
14	$9\frac{40}{48}$	$137\frac{3}{4}$
15	$9\frac{10}{48}$	138
16	$8\frac{34}{48}$	$138\frac{1}{4}$
17	$8\frac{9}{48}$	$139\frac{1}{5}$ max.
18	$7\frac{32}{48}$	138
20	——	Stopped the wheel.

Comparing the power and effect, the ratio is as 10 is to 7·65, when the height of the wheel is taken from the quantity of fall, and the nature of the experiment does not afford the means of knowing accurately the whole height. This ratio agrees very well with the result of Smeaton's trials.

It appears from both the experiments of Smeaton and Bossut that the load corresponding to the maximum is $\frac{6}{7}$ of the load that stops the wheel.—Ed.

PART III.

ON THE CONSTRUCTION AND EFFECTS OF WINDMILL SAILS.

Read before the Royal Society, May 31 *and June* 14, 1759.

In trying experiments on windmill sails, the wind itself is too uncertain to answer the purpose, we must, therefore, have recourse to an artificial wind.

This may be done two ways; either by causing the air to move against the machine, or the machine to move against the air. To cause the air to move against the machine, in a sufficient volume, with steadiness and the requisite velocity, is not easily put in practice: to carry the machine forward in a right line against the air would require a larger room than I could conveniently meet with. What I found most practicable, therefore, was to carry the axis, whereon the sails were to be fixed, progressively round in the circumference of a large circle. Upon this idea * a machine was constructed, as follows:

* Some years ago Mr. Rouse, an ingenious gentleman of Harborough, in Leicestershire, set about trying experiments on the velocity of the wind, and force

PLATE III. Fig. 1.

A B C is a pyramidical frame for supporting the moving parts.

D E is an upright axis, whereon is framed

F G, an arm for carrying the sails at a proper distance from the centre of the upright axis.

H is a barrel upon the upright axis, whereon is wound a cord; which, being drawn by the hand, gives a circular motion to the axis, and to the arm F G, and thereby carries the axis of the sails in the circumference of a circle, whose radius is D I, causing thereby the sails to strike the air, and turn round upon their own axis.

At L is fixed the end of a small line which, passing through the pullies M N O, terminates upon a small cylinder or barrel upon the axis of the sails, and, by winding thereon, raises

P, the scale, wherein the weights are placed for trying the power of the sails. This scale, moving up and down

thereof upon plain surfaces and windmill sails; and, much about the same time, Mr. Ellicott contrived a machine for the use of the late celebrated Mr. B. Robins, for trying the resistance of plain surfaces moving through the air. The machines of both these gentlemen were much alike, though at that time totally unacquainted with each other's inquiries. But it often happens that when two persons think justly upon the same subject, their experiments are alike. This machine was also built upon the same idea as the foregoing, but differed in having the hand for the first mover, with a pendulum for its regulator, instead of a weight, as in the former, which was certainly best for the purposes of measuring the impulse of the wind, or resistance of planes; but the latter is more applicable to experiments on windmill sails, because every change of position of the same sails will occasion their meeting the air with a different velocity, though urged by the same weight.

in the direction of the upright axis, receives no disturbance from the circular motion.

Q R two parallel pillars standing upon the arm F G, for the purpose of supporting and keeping steady the scale P; which is kept from swinging by means of

S T, two small chains, which hang loosely round the two pillars.

W is a weight for bringing the centre of gravity of the movable part of the machine into the centre of motion of the axis D E.

V X is a pendulum, composed of two balls of lead, which are movable upon a wooden rod, and thereby can be so adjusted, as to vibrate in any time required. This pendulum hangs upon a cylindrical wire, whereon it vibrates, as on a rolling axis.

Y is a perforated table for supporting the axis of the pendulum.

Note. The pendulum being so adjusted, as to make two vibrations in the time that the arm F G is intended to make one turn; the pendulum being set a vibrating, the experimenter pulls by the cord Z, with sufficient force to make each half revolution of the arm to correspond with each vibration, as equally as possible, during the number of vibrations that the experiment is intended to be continued. A little practice renders it easy to give motion thereto with all the regularity that is necessary.

Specimen of a Set of Experiments.

Radius of the sails ..... 21 inches
Length of ditto in the cloth 18
Breadth of ditto ..... 5·6
* Angle at the extremity ..... 10 degrees
Ditto at the greatest inclination 25
20 turns of the sails raised the weight ..... 11·3 inches
Velocity of the centre of the sails, in the circumference of the great circle, in a second ..... 6 feet 0 inches
Continuance of the experiment ..... 52 seconds

No.	Wt. in the Scale.	Turns.	Product.
1	0 *lb.*	108	0
2	6	85	510
3	6½	81	526½
4	7	78	546
5	7½	73	547½ maximum.
6	8	65	520
7	9	0	0

N. B. The weight of the scale and pulley was 3 oz.; and that 1 oz. suspended upon one of the radii, at 12½ inches from the centre of the axis, just overcame the friction; scale, and load of 7½ lbs.; and placed at $14\frac{19}{20}$ inches, overcame the same resistance with 0 lbs. in the scale.

Reduction of the preceding Specimen.

No 5 being taken for the *maximum*, the weight in the scale was 7 lbs. 8 oz., which, with the weight of the scale and

* In all the following experiments the angle of the sails is accounted from the plane of their motion; that is, when they stand at right angles to their axis, their angle is denoted 0°, this notation being agreeable to the language of practitioners, who call the angle so denoted the weather of the sail; which they denominate greater or less, according to the quantity of this angle.

pulley, 3 oz. makes 7 lbs. 11 oz. equal to 123 oz.; this added to the friction of the machinery, the sum is the whole resistance.* The friction of the machinery is thus deduced: since 20 turns of the sails raised the weight 11·3 inches, with a double line, the radius of the cylinder will be ·18 of an inch; but, had the weight been raised by a single line, the radius of the cylinder being half the former, viz. ·09, the resistance would have been the same. We shall, therefore, have this analogy: as half the radius of the cylinder is to the length of the arm where the small weight was applied, so is the weight applied to the arm to a fourth weight, which is equivalent to the sum of the whole resistance together; that is, ·09 : 12·5 : : 1 oz. : 139 oz.; this exceeds 123 oz., the weight in the scale, by 16 oz. or 1 lb., which is equivalent to the friction; and which, added to the above weight of 7 lbs. 11 oz. makes 8 lbs. 11 oz. = 8·69 lbs. for the sum of the whole resistance; and this, multiplied by 73 turns, makes a product of 634, which may be called the representative of the *effect* produced.

In like manner, if the weight 9 lbs. which caused the sails to rest after being in motion, be augmented by the weight of the scale and its relative friction, it will become 10·37 lbs. The result of this specimen is set down in No. 12. of Tab. III. and the results of every other set of experiments therein contained were made and reduced in the same manner.

* The resistance of the air is not taken into the account of resistance, because it is inseparable from the application of the power.

Specimen of a Set of Experiments.

Radius of the sails ..... 21 in
Length of ditto in the cloth 18
Breadth of ditto ..... 5·6
* Angle at the extremity ..... 10 de
* Ditto at the greatest inclination 25
20 turns of the sails raised the weight ..... 11·3
Velocity of the centre of the sails, in the circumference of the great circle, in a second ..... 6 feet 0 in
Continuance of the experiment ..... 52 se

No.	Wt. in the Scale.	Turns.	Product.
1	0 *lb.*	108	0
2	6	85	510
3	6½	81	526½
4	7	78	546
5	7½	73	547½ ma
6	8	65	520
7	9	0	0

N. B. The weight of the scale and pulley was 3 oz.; a 1 oz. suspended upon one of the radii, at 12½ inches fr centre of the axis, just overcame the friction; scale, and 7½ lbs.; and placed at 14$\frac{19}{20}$ inches, overcame the same res with 9 lbs. in the scale.

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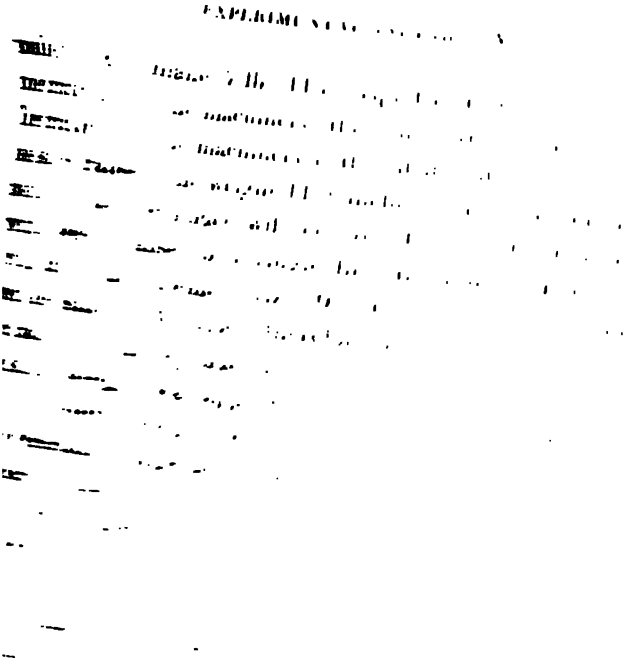

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TABLE III.

Containing Nineteen Sets of Experiments on Windmill Sails of various Structures, Positions, and Quantities of Surface.

The kind of sails made use of.	No.	Angle at the extremities.	Greatest angle.	Turns of the sails unloaded.	Turns of ditto at the maximum.	Load at the maximum.	Greatest load.	Product.	Quantity of surface.	Ratio of greatest velocity to the velocity at maximum.	Ratio of greatest load to the load at maximum.	Ratio of surface to the product.
		°	°			*lbs.*	*lbs.*		*Sq.In.*			
Plain sails at an angle of 55° with the axis	1	35	35	66	42	7·56	12.59	318	404	10:7	10:6	10:7·9
Plain sails weathered according to the common practice.	2	12	12		70	6·3	7·56	441	404		10:8·3	10:10·1
	3	15	15	105	69	6·72	8·12	464	404	10:6·6	10:8·3	10:10·15
	4	18	18	96	66	7·0	9·81	462	404	10:7	10:7·1	10:10·15
Weathered according to *Maclaurin's theorem.*	5	9	26½		66	7·0		462	404			10:11·4
	6	12	29½		70½	7·35		518	404			10:12·8
	7	15	32½		63½	8·3		527	404			10:13
Sails weathered in the *Dutch* manner, tried in various positions.	8	0	15	120	93	4·75	5·31	442	404	10:7·7	10:8·9	10:11
	9	3	18	120	79	7·0	8·12	553	404	10:6·6	10:8·6	10:13·7
	10	5	20		78	7·5	8·12	585	404		10:9·2	10:14·5
	11	7½	22½	113	77	8·3	9·81	639	404	10:6·8	10:8·5	10:15·8
	12	10	25	108	73	8·69	10·37	634	404	10:6·8	10:8·4	10:15·7
	13	12	27	100	66	8·41	10·94	580	404	10:6·6	10:7·7	10:14·4
Sails weathered in the *Dutch* manner, but *enlarged* towards the extremities.	14	7½	22½	123	75	10·65	12·59	799	505	10:6·1	10:8·5	10:15·8
	15	10	25	117	74	11·08	13·69	820	505	10:6·3	10:8·1	10:16·2
	16	12	27	114	66	12·09	14·23	799	505	10:5·8	10:8·4	10:15·8
	17	15	30	96	63	12·09	14·78	762	505	10:6·6	10:8·2	10:15·1
3 sails being *sectors of ellipses* in their best positions.	18	12	22	105	64½	16·42	27·87	1059	854	10:6·1	10:5·9	10:12·4
	19	12	22	99	64½	18·06		1165	1146	10:5 9		10:10·1
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.

OBSERVATIONS AND DEDUCTIONS FROM THE PRECEDING EXPERIMENTS.

I. *Concerning the best Form and Position of Windmill Sails.*

In Tab. III. No. 1, is contained the result of a set of experiments upon sails set at the angle which the celebrated Mons. Parent, and succeeding geometricians for many years, held to be the best; viz. those whose planes make an angle of 55°, nearly, with the axis; the complement whereof, or angle that the plane of the sail makes with the plane of their motion, will therefore be 35°, as set down in cols. 2 and 3. Now, if we multiply their number of turns by the weight they lifted, when working to the greatest advantage, as set down in cols. 5 and 6, and compare this product (col. 8) with the other products contained in the same column, instead of being the greatest, it turns out the least of all the rest. But if we set the angle of the same planes at somewhat less than half the former, or at any angle from 15° to 18°, as in Nos. 3 and 4, that is, from 72° to 75° with the axis, the product will be increased in the ratio of 31 : 45; and this is the angle most commonly made use of by practitioners, when the surfaces of the sails are planes.

If nothing more was intended than to determine the most efficacious angle to make a mill acquire motion from a state of rest, or to prevent it from passing into rest from a state of motion, we shall find the position of No. 1 the best; for if we consult col. 7, which contains the least weights that would make the sails pass from motion to rest, we shall find that of No. 1 (relative to the quantity of cloth) the greatest of all. But if the sails are intended, with given dimensions, to produce the greatest effect possible in a given time, we must entirely reject

those of No. 1, and *if we are confined to the use of planes, conform ourselves to some angle between* Nos. 3 *and* 4, *that is not less than* 72°, *or greater than* 75°, *with the axis.*

The late celebrated Mr. Maclaurin has judiciously distinguished between the action of the wind upon a sail at rest, and a sail in motion: and, in consequence, as the motion is more rapid near the extremities than towards the centre, that the angle of the different parts of the sail, as they recede from the centre, should be varied. For this purpose he has furnished us with the following theorem.* "Suppose the velocity of the wind to be represented by a, and the velocity of any given part of the sail to be denoted by c; then the effort of the wind upon that part of the sail will be greatest, when the tangent of the angle, in which the wind strikes it, is to radius as $\frac{3c}{2a}+\sqrt{2+\frac{9c^2}{4a^2}}$ to 1." This theorem then assigns the law, by which the angle is to be varied according to the velocity of each part of the sail to the wind: but as it is left undetermined what velocity any one given part of the sail ought to have in respect to the wind, the angle that any one part of the sail ought to have, is left undetermined also; so that we are still at a loss for the proper *data* to apply the theorem. However, being willing to avail myself thereof, and considering that any angle from 15° to 18° was best suited to a plane, and, of consequence, the best mean angle, I made the sail, at the middle distance between the centre and the extremity, to stand at an angle of 25° 41′ with the plane of the motion; in which case the velocity of that part of the sail, when loaded to a *maximum*, would be equal to that of the wind, or $c = a$. This being determined, the rest were inclined according to the theorem, as follows:—

* Maclaurin's Account of Sir Isaac Newton's Philosophical Discoveries, p. 176, art. 29.

Parts of the radius from the centre.		Angle with the axis.	Angle of weather.	
$\frac{1}{6}$	$c = \frac{1}{3}a$	63° 26′	26° 34′	
$\frac{2}{6}$	$c = \frac{2}{3}a$	69 54	20 6	
$\frac{1}{2}$	$c = a$	74 19	15 41	middle.
$\frac{2}{3}$	$c = 1\frac{1}{3}a$	77 20	12 40	
$\frac{5}{6}$	$c = 1\frac{2}{3}a$	79 27	10 33	
1	$c = 2a$	81 0	9 0	extremity.

The result hereof was according to No. 5, being nearly the same as the plane sails, in their best position: but being turned round in their sockets, so that every part of each sail stood at an angle of 3°, and afterwards at 6°, greater than before, that is, their extremities being moved from 9° to 12° and 15°, the products were advanced to 518 and 527 respectively. Now, from the small difference between those two products, we may conclude, that they were nearly in their best position, according to No. 7, or some angle between that and No. 6; but from these, as well as the plane sails and others, we may also conclude, that *a variation in the angle of a degree or two makes very little difference in the effect, when the angle is near upon the best.*

It is to be observed, that a sail inclined by the preceding rule will expose a convex surface to the wind: whereas the Dutch, and all our modern mill-builders, though they make the angle to diminish, in receding from the centre towards the extremity, yet constantly do it in such a manner, as that the surface of the sail may be concave towards the wind. In this manner the sails made use of in Nos. 8, 9, 10, 11, 12, and 13, were constructed; the middle of the sail making an angle with the extreme bar of 12°; and the greatest angle (which was about one-third of the radius from the centre) of 15° therewith. Those sails being tried in various positions, the best appears to be that of No. 11, where the extremities stood at an angle of $7\frac{1}{2}$° with the plane of motion, the product being 639: greater

than than that of those made by the theorem in the ratio of 9 : 11, and double to that of No. 1; and this was the greatest product that could be procured without an augmentation of surface. Hence it appears, *that when the wind falls upon a concave surface, it is an advantage to the power of the whole, though every part, taken separately, should not be disposed to the best advantage.**

Having thus obtained the best position of the sails, or manner of weathering, as it is called by the workmen, the next point was to try what advantage could be made by an addition of surface upon the same radius. For this purpose, the sails made use of had the same weather as those Nos. 8 to 13, with an addition to the leading side of each of a triangular cloth, whose height was equal to the height of the sail, and whose base was equal to half the breadth: of consequence, the increase of surface upon the whole was one-fourth part, or as 4:5. Those sails, by being turned round in their sockets, were tried in four different positions, specified in Nos. 14, 15, 16, and 17; from whence it appears, that the best was when every part of the sail made a greater angle, by $2\frac{1}{2}°$, with the plane of the motion, than those without the addition, as appears by No. 15, the product being 820: this exceeds 639 more than in the ratio of 4 : 5, or that of the increase of cloth. Hence it appears, that *a broader sail requires a greater angle;* and *that*

* By several trials in large, I have found the following angles to answer as well as any. The radius is supposed to be divided into 6 parts; and 1-6th, reckoning from the centre, is called 1, the extremity being denoted 6.

No.	Angle with the axis.	Angle with the plane of the motion.
1	72°	18°
2	71	19
3	72	18 middle.
4	74	16
5	77½	12½
6	83	7 extremity.

*when the sail is broader at the extremity, than near the centre, this shape is more advantageous than that of a parallelogram.**

Many have imagined, that the more sail the greater the advantage, and have, therefore, proposed to fill up the whole area: and by making each sail a sector of an ellipsis, according to Monsieur Parent, to intercept the whole cylinder of wind, and thereby to produce the greatest effect possible.

We have, therefore, proceeded to inquire how far the effect could be increased by a further enlargement of the surface, upon the same radius of which Nos. 18 and 19 are specimens. The surfaces, indeed, were not made planes, and set at an angle of 35°, as Parent proposed; because, from No. 1, we learn, that this position has nothing to do, when we intend them to work to the greatest advantage. We, therefore, gave them such an angle as the preceding experiments indicated for such sort of sails, viz. 12° at the extremity, and 22° for the greatest weather. By No. 18, we have the product 1059, greater than No. 15, in the ratio of 7 : 9; but then the augmentation of cloth is almost 7 : 12. By No. 19, we have the product 1165, that is greater than No. 15, as 7 : 10; but the augmentation of cloth is nearly as 7 : 16; consequently, had the same quantity of cloth as in No 18, been disposed in a figure, similar to that of No. 15, instead of the product 1059, we should have had the product 1386; and in No. 19, instead of the product 1165, we should have had a product of 1860; as will be further made appear

* The figure and proportion of the enlarged sails, which I have found best to answer in large, are represented in the figure, Plate III., where the extreme bar is 1-3d of the radius (or whip, as it is called by the workmen), and is divided by the whip in the proportion of 3 to 5. The triangular, or leading sail, is covered with board, from the point downward, 1-3d of its height, the rest with cloth, as usual. The angles of weather in the preceding note are best for the enlarged sails also; for, in practice, it is found that the sails had better have too little than too much weather.

in the course of the following deductions. Hence it appears, that beyond a certain degree, the more the area is crowded with sail, the less effect is produced in proportion to the surface: and by pursuing the experiments still further, I found, that though in No. 19, the surface of all the sails together were not more than 7-8ths of the circular area containing them, yet a further addition rather diminished than increased the effect. *So that when the whole cylinder of wind is intercepted, it does not then produce the greatest effect, for want of proper interstices to escape.*

It is certainly desirable that the sails of windmills should be as short as possible; but at the same time it is equally desirable, that the quantity of cloth should be the least that may be, to avoid damage by sudden squalls of wind. The best structure, therefore, for large mills is that where the quantity of cloth is the greatest, in a given circle, that can be: on this condition, that the effect holds out in proportion to the quantity of cloth; for otherwise the effect can be augmented in a given degree by a lesser increase of cloth upon a larger radius, than would be required, if the cloth was increased upon the same radius. The most useful figure, therefore, for practice, is that of No. 9 or 10, as has been experienced upon several mills in large.

TABLE IV.

Containing the Result of Six Sets of Experiments, made for determining the Difference of Effect according to the different Velocity of the Wind.

N.B. The sails were of the same size and kind as those of Nos. 10, 11, and 12, Tab. III. Continuance of the Experiment one minute.

No.		Angle at the extremity.	Velocity of the wind in a second.		Turns of the sails unloaded.	Turns of the sails at maximum.	Load at the maximum.	Greatest load.	Product.	Maximum load for the half velocity.	Turns of the sails therewith.	Product of lesser load and greater velocity.	Ratio of the two products.	Ratio of the greatest velocity to the velocity at a maximum.	Ratio of the greatest load to the load at a maximum.	No.
			f.	in.			lbs.	lbs.								
1		5°	4		96	66	4·47	5·37	295	—	—	—	—	10 : 6·9	10 : 8·3	1
2		5	8	9	207	122	16·42	18·06	2003	4·47	180	805	10 : 27·3	10 : 5·9	10 : 9·1	2
3		7½	4	4½	—	65	4·62	—	300	—	—	—	—	—	—	3
4		7½	8	9	—	130	17·52	—	2278	4·62	180	832	10 : 27·8	—	—	4
5		10	4	4½	91	61	5·03	5·87	307	—	—	—	—	10 : 6·7	10 : 8·5	5
6		10	8	9	178	110	18·61	21·34	2047	5·03	158	795	10 : 26	10 : 6·2	10 : 8·7	6
		2	3		4	5	6	7	8	9	10	11	12	13	14	

II.—*Concerning the Ratio between the Velocity of Windmill Sails unloaded, and their Velocity when loaded to a Maximum.*

Those ratios, as they turned out in experiments upon different kinds of sails, and with different inclinations (the velocity of the wind being the same), are contained in col. 10 of Tab. III. where the extremes differ from the ratio of 10 : 7·7 to that of 10 : 5·8 ; but *the most general ratio of the whole will be nearly as* 3 : 2. This ratio also agrees sufficiently near with experiments where the velocity of the wind was different, as in those contained in Tab. IV. col. 13. in which the ratios differ from 10 : 6·9 to that of 10 : 5·9. However, it appears, in general, that where the power is greater, whether by an enlargement of surface, or a greater velocity of the wind, that the second term of the ratio is less.

III.—*Concerning the Ratio between the greatest Load that the Sails will bear without stopping, or what is nearly the same thing, between the least Load that will stop the Sails, and the Load at the Maximum.*

Those ratios for different kinds of sails and inclinations, are collected in col. 11. Tab. III. where the extremes differ from the ratio of 10 : 6 to that of 10 : 9·2 ; but taking in those sets of experiments only, where the sails respectively answered best, *the ratios will be confined between that of* 10 : 8 *and of* 10 : 9 ; *and at a medium about* 10 : 8·3 *or* 6 : 5. This ratio also agrees nearly with those in col. 14. of Tab. IV. However it appears, upon the whole, that in those instances where the angle of the sails or quantity of cloth were greatest, that the second term of the ratio was less.

IV.—*Concerning the Effects of Sails, according to the different Velocity of the Wind.*

Maxim 1.—*The velocity of windmill sails, whether unloaded or loaded, so as to produce a maximum, is nearly as the velocity of the wind, their shape and position being the same.*

This appears by comparing together the respective numbers of cols. 4 and 5, Tab. IV., wherein those of Nos. 2, 4, and 6, ought to be double of Nos. 1, 3, and 5: but as the deviation is nowhere greater than what may be imputed to the inaccuracy of the experiments themselves, and holds good exactly in Nos. 3 and 4; which sets were deduced from the medium of a number of experiments, carefully repeated the same day, and, on that account, are most to be depended upon, we may therefore conclude the maxim true.

Maxim 2.—*The load at the maximum is nearly, but somewhat less than, as the square of the velocity of the wind, the shape and position of the sails being the same.*

This appears by comparing together the numbers in col. 6, Tab. IV., wherein those of Nos. 2, 4, and 6 (as the velocity is double) ought to be quadruple of those Nos. 1, 3, and 5; instead of which they fall short, No. 2 by $\frac{1}{14}$, No. 4 by $\frac{1}{19}$, and Nos. 6 by $\frac{1}{13}$ part of the whole. The greatest of those deviations is not more considerable than might be imputed to the unavoidable errors in making the experiments: but as those experiments, as well as those of the greatest load, all deviate the same way, and also coincide with some experiments communicated to me by Mr. Rouse, upon the resistance of planes, I am led to suppose a small deviation, whereby the load falls short of the squares of the velocity; and, since the experiments, Nos. 3 and 4, are most to be depended upon, we must

be as the square of the velocity of the wind; and the effect, being, in this case, as the turns of the sails, will be as the square of the velocity of the wind also; but this must be understood only of the first increments of the velocity of the wind: for,

2dly, As the sails will never acquire above a given velocity in relation to the wind, though the load was diminished to nothing, when the load continues the same, the more the velocity of the wind increases (though the effect will continue to increase) yet the more it will fall short of the square of the velocity of the wind; so that when the velocity of the wind is double, the increase of effect, instead of being as 1 : 4, according to the squares, it turns out as 10 : 27½, as thus appears. In Tab. IV., col. 9, the loads of Nos. 2, 4, and 6, are the same as the maximum loads in col. 6. of Nos. 1, 3, and 5. The number of turns of the sails with those loads, when the velocity of the wind is double, are set down in col. 10, and the products of their multiplication in col. 11: those being compared with the products of Nos. 1, 3, and 5, col. 8. furnish the ratios set down in col. 12. which, at a medium (due regard being had to Nos. 3 and 4) will be nearly as 10 : 27½. 3dly, The load continuing the same, grows more and more inconsiderable, respecting the power of the wind as it increases in velocity; so that the turns of the sails grow nearer and nearer a coincidence with their turns unloaded; that is, nearer and nearer to the simple ratio of the velocity of the wind. When the velocity of the wind is double, the turns of the sails, when loaded to a *maximum*, will be double also; but, *unloaded*, will be more than triple, by deduction 2d: and, therefore, the product could not have increased beyond the ratio of 10 : 30 (instead of 10 : 27½), even supposing the sails not to have been retarded at all by carrying the *maximum* load for half the velocity. Hence we see, that when the velocity of the wind exceeds the double of that, where a constant load produces a *maximum*, that the increase of effect, which follows the increase of the velocity of the sails, will be nearly as the velocity of the wind, and ultimately in that

ratio precisely. Hence, also, we see that windmills, such as the different species for raising water for drainage, &c. lose much of their full effect, when acting against one invariable opposition.

V.—*Concerning the Effects of Sails of different Magnitudes, the Structure and Position being similar, and the Velocity of the Wind the same.*

Maxim 6.—*In sails of a similar figure and position, the number of turns in a given time will be reciprocally as the radius or length of the sail.*

The extreme bar having the same inclination to the plane of its motion, and to the wind: its velocity at a *maximum* will always be in a given ratio to the velocity of the wind; and, therefore, whatever be the radius, the absolute velocity of the extremity of the sail will be the same; and this will hold good respecting any other bar, whose inclination is the same, at a proportionable distance from the centre; it therefore follows, that the extremity of all similar sails, with the same wind, will have the same absolute velocity; and, therefore, take a space of time to perform one revolution in proportion to the radius; or, which is the same thing, the number of revolutions in the same given time, will be reciprocally as the length of the sail.

Maxim 7.—*The load at a maximum that sails of a similar figure and position will overcome, at a given distance from the centre of motion, will be as the cube of the radius.*

Geometry informs us, that in similar figures the surfaces are as the squares of their similar sides; of consequence the quantity of cloth will be as the square of the radius: also, in similar figures and positions, the impulse of the wind upon every similar section of the cloth, will be in proportion to the surface

of that section; and, consequently, the impulse of the wind upon the whole, will be as the surface of the whole: but as the distance of every similar section, from the centre of motion, will be as the radius; the distance of the centre of power of the whole, from the centre of motion, will be as the radius also: that is, the lever by which the power acts will be as the radius: as, therefore, the impulse of the wind, respecting the quantity of cloth, is as the square of the radius, and the lever by which it acts, as the radius simply; it follows, that the load which the sails will overcome, at a given distance from the centre, will be as the cube of the radius.

Maxim 8.—*The effect of sails of similar figure and position, are as the square of the radius.*

By Maxim 6, it is proved, that the number of revolutions made in a given time, are as the radius inversely. Under Maxim 7, it appears, that the length of the lever, by which the power acts, is as the radius directly; therefore these equal and opposite ratios destroy one another: but, as in similar figures the quantity of cloth is as the square of the radius, and the action of the wind is in proportion to the quantity of cloth, as also appears under Maxim 7, it follows that the effect is as the square of the radius.

Corol. 1. Hence it follows, that augmenting the length of the sail, without augmenting the quantity of cloth, does not increase the power; because what is gained by the length of the lever, is lost by the slowness of the rotation.

Corol. 2. If the sails are increased in length, the breadth remaining the same, the effect will be as the radius.

VI.—*Concerning the Velocity of the Extremities of Windmill Sails, in respect to the Velocity of the Wind.*

Maxim 9.—*The velocity of the extremities of* Dutch *sails, as well as of the enlarged sails, in all their usual positions when unloaded, or even loaded to a maximum, is considerably quicker than the velocity of the wind.*

The *Dutch* sails unloaded, as in Tab. III. No. 8, made 120 revolutions in 52″: the diameter of the sails being 3 feet 6 inches, the velocity of their extremities will be 25·4 feet in a second; but the velocity of the wind producing it, being 6 feet in the same time, we shall have 6 : 25·4 :: 1 : 4·2; in this case, therefore, the velocity of their extremities was 4·2 times greater than that of the wind. In like manner, the relative velocity of the wind, to the extremities of the same sails, when loaded to a *maximum*, making then 93 turns in 52″, will be found to be as 1 : 3·3; or 3·3 times quicker than that of the wind.

The following table contains 6 examples of *Dutch* sails, and four examples of the enlarged sails, in different positions, but with the constant velocity of the wind of 6 feet in a second, from Tab. III.; and also 6 examples of *Dutch* sails in different positions, with different velocities of the wind from Tab. IV.

TABLE V.

Containing the Ratio of the Velocity of the Extremities of Windmill Sails to the Velocity of the Wind.

No.	No. of Table III. and IV.	Angle at the Extremity.	Velocity of the wind in a second.	Ratio of the velocity of the wind and extremities of the sails.		
				unloaded.	loaded.	
1	8	0°	6 0	1 : 42	1 : 3·3	From Table III.
2	9	3	6 0	1 : 4·2	1 : 2·8	
3	10	5	6 0	— —	1 : 2·75	
4	11	7½	6 0	1 : 4	1 : 2·7	
5	12	10	6 0	1 : 3·8	1 : 2·6	
6	13	12	6 0	1 : 3·5	1 : 2·3	
7	14	7½	6 0	1 : 4·3	1 : 2·6	
8	15	10	6 0	1 : 4·1	1 : 2·6	
9	16	12	6 0	1 : 4	1 : 2·3	
10	17	15	6 0	1 : 3·35	1 : 2·2	
11	1	5	4 4½	1 : 4	1 : 2·8	From Table IV.
12	2	5	8 9	1 : 4·3	1 : 2·6	
13	3	7½	4 4½	— —	1 : 2·8	
14	4	7½	8 9	— —	1 : 2·7	
15	5	10	4 4½	1 : 3·8	1 : 2·6	
16	6	10	8 9	1 : 3·4	1 : 2·3	
1	2	3	4	5	6	

It appears from the preceding collection of examples, that when the extremities of the *Dutch* sails are parallel to the plane of motion, or at right angles to the wind and to the axis, as they are made according to the common practice in *England*, that their velocity, unloaded, is above four times, and loaded to a *maximum*, above three times greater than that of the wind: but that when the Dutch sails, or enlarged sails, are

in their best positions, their velocity unloaded is four times, and loaded to a *maximum*, at a medium, the Dutch sails are 2·7, and the enlarged sails 2·6 times greater than the velocity of the wind. Hence we are furnished with a method of knowing the velocity of the wind, from observing the velocity of the windmill sails: for, knowing the radius and the number of turns in a minute, we shall have the velocity of the extremities; which, divided by the following divisors, will give the velocity of the wind.

Dutch sails in their common position	unloaded	4·2
	loaded..	3·3
Dutch sails in their best position....	unloaded	4·0
	loaded..	2·7
Enlarged sails in their best position..	unloaded	4·0
	loaded..	2·6

From the above divisors there arises the following compendiums: supposing the radius to be 30 feet, which is the most usual length in this country, and the mill to be loaded to a *maximum*, as is usually the case with corn mills; for every 3 *turns in a minute, of the Dutch sails in their common position, the wind will move at the rate of two miles an hour;* for every 5 *turns in a minute of the Dutch sails in their best position, the wind moves four miles an hour;* and for every 6 *turns in a minute, of the enlarged sails in their best position, the wind will move five miles an hour.*

The following table, which was communicated to me by my friend, Mr. Rouse, and which appears to have been constructed with great care, from a considerable number of facts and experiments, and which, having relation to the subject of this article, I here insert it as he sent it to me; but, at the same time, must observe, that the evidence for those numbers where the velocity of the wind exceeds 50 miles in an hour, does not seem of equal authority with those of 50 miles an hour and under It is also to

be observed, that the numbers in col. 3, are calculated according to the square of the velocity of the wind, which, in moderate velocities, from what has been before observed, will hold very nearly.

TABLE VI.

Containing the Velocity and Force of Wind, according to their common Appellations.

Velocity of the wind.		Perpendicular force on one foot area in pounds avoirdupoise.	Common appellations of the force of winds.
Miles in one hour.	Feet in one second.		
1	1·47	·005	Hardly perceptible.
2	2·93	·020	Just perceptible.
3	4·40	·044	
4	5·87	·079	Gentle pleasant wind.
5	7·33	·123	
10	14·67	·492	Pleasant brisk gale.
15	22·00	1·107	
20	29·34	1·968	Very brisk.
25	36·67	3·075	
30	44·01	4·429	High winds.
35	51·34	6·027	
40	58·68	7·873	Very high.
45	66·01	9·963	
50	73·35	12·300	A storm or tempest.
60	88·02	17·715	A great storm.
80	117·36	31·490	A hurricane.
100	146·70	49·200	A hurricane that tears up trees, carries buildings before it, &c.
1	2	3	

VII.— *Concerning the absolute Effect produced by a given Velocity of the Wind upon Sails of a given Magnitude and Construction.*

It has been observed by practitioners, that, in mills with Dutch sails in the common position, when they make about 13 turns in a minute, they then work at a mean rate: that is, by the compendiums in the last article, when the velocity of the wind is $8\frac{2}{3}$ miles an hour, or $12\frac{2}{3}$ feet in a second; which, in common phrase, would be called a *fresh gale*.

The experiments set down in Tab. IV. No. 4. were tried with a wind, whose velocity was $8\frac{1}{4}$ feet in a second; consequently, had those experiments been tried with a wind whose velocity was $12\frac{2}{3}$ feet in a second, the effect, by Maxim 3d, would have been 3 times greater: because the cube of $12\frac{2}{3}$ is 3 times greater than that of $8\frac{1}{4}$.

From Tab. IV. No. 4. we find that the sails, when the velocity of the wind was $8\frac{1}{4}$ feet in a second, made 130 revolutions in a minute, with a load of 17·52 lbs. From the measures of the machine preceding the specimen of a set of experiments, we find, that twenty revolutions of the sails raised the scale and weight 11·3 inches: 130 revolutions will therefore raise the scale 73·45 inches, which, multiplied by 17·52 lbs., makes a product of 1287, for the effect of the Dutch sails in their best position; that is, when the velocity of the wind is $8\frac{1}{4}$ feet in a second: this product, therefore, multiplied by three, will give 3861 for the effect of the same sails, when the velocity of the wind is $12\frac{2}{3}$ feet in a second.

Desaguliers makes the utmost power of a man, when working so as to be able to hold it for some hours, to be equal to that

of raising a hogshead of water 10 feet high in a minute. Now, a hogshead, consisting of 63 ale gallons, being reduced into pounds avoirdupois, and the height into inches; the product made by multiplying those two numbers will be 76,800; which is 19 times greater than the product of the sails last mentioned, at $12\frac{1}{2}$ feet in a second: therefore, by Maxim 8th, if we multiply the square root of 19, that is 4·46, by 21 inches, the length of the sail producing the effect 3861, we shall have 93·66 inches, or 7 feet $9\frac{2}{3}$ inches for the radius of a Dutch sail in its best position, whose mean power shall be equal to that of a man: but if they are in their common position, their length must be increased in the ratio of the square root of 442 to that of 639, as thus appears:

The ratio of the *maximum* products of Nos. 8 and 11, Tab. III. are as 442 : 639 : but, by Maxim 8, the effects of sails of different radii are as the square of the radii; consequently, the square roots of the products or effects, are as the radii simply: and, therefore, as the square root of 442 is to that of 639, so is 93·66 to 112·66; or 9 feet $4\frac{2}{3}$ inches.

If the sails are of the enlarged kind, then, from Tab. III. Nos. 11 and 15, we shall have the square root of 820 to that of 639 : : 93·66 : 82·8 inches, or 6 feet $10\frac{3}{4}$ inches: so that, in round numbers, we shall have the radius of a sail, of a similar figure to their respective models, whose mean power shall be equal to that of a man:

The Dutch sails in their common position	$9\frac{1}{2}$ feet
The Dutch sails in their best position.....	8
The enlarged sails in their best position.....	7

Suppose, now, the radius of a sail to be 30 feet, and to be constructed upon the model of the enlarged sails, No. 14 or 15, Tab. III. dividing 30 by 7, we shall have 4·28, the square of which is 18·3; and this, according to Maxim 7, will be the relative

power of a sail of 30 feet to one of 7 feet; that is, when working at a mean rate, the 30 feet sail will be equal to the power of 18·3 men, or of $3\frac{2}{3}$ horses; reckoning 5 men to a horse: whereas the effect of the common Dutch sails, of the same length, being less in the proportion of 820 : 442, will be scarce equal to the power of 10 men, or of 2 horses.

That these computations are not merely speculative, but will nearly hold good when applied to works in large, I have had an opportunity of verifying: for, in a mill with the enlarged sails of 30 feet, applied to the crushing of rape-seed, by means of two runners upon the edge, for making oil, I observed, that when the sails made 11 turns in a minute, in which case the velocity of the wind was about 13 feet in a second, according to Article VI., that the runners then made 7 turns in a minute: whereas 2 horses, applied to the same two runners, scarcely worked them at the rate of $3\frac{1}{2}$ turns in the same time. Lastly, with regard to the real superiority of the enlarged sails above the Dutch sails as commonly made, it has sufficiently appeared, not only in those cases where they have been applied to new mills, but where they have been substituted in the place of the others.

VIII.—*Concerning horizontal Windmills and Waterwheels with oblique Vanes.*

Observations upon the effects of common windmills, with oblique vanes, have led many to imagine that, could the vanes be brought to receive the direct impulse, like a ship sailing before the wind, it would be a very great improvement in point of power; while others, attending to the extraordinary and even unexpected effects of oblique vanes, have been led to imagine that oblique vanes applied to watermills, would as much exceed the common waterwheels, as the vertical windmills are found to have exceeded all attempts towards a horizontal one. Both these notions, but especially the first, have so

plausible an appearance, that of late years there have seldom been wanting those who have assiduously employed themselves to bring to bear designs of this kind; it may not, therefore, be unacceptable to endeavour to set this matter in a clear light.

Plate III. *Fig.* 2. Let A B be the section of a plane, upon which let the wind blow in the direction C D, with such a velocity as to describe a given space B E, in a given time (suppose one second), and let A B be moved parallel to itself, in the direction C D. Now, if the plane A B moves with the same velocity as the wind; that is, if the point B moves through the space B E in the same time that a particle of air would move through the same space; it is plain that, in this case, there can be no pressure or impulse of the wind upon the plane: but if the plane moves slower than the wind, in the same direction, so that the point B may move to F, while a particle of air, setting out from B at the same instant, would move to E, then B F will express the velocity of the plane; and the relative velocity of the wind and plane will be expressed by the line F E. Let the ratio of F E to B E be given (suppose 2 : 3), let the line A B represent the impulse of the wind upon the plane A B, when acting with its whole velocity B E; but, when acting with its relative velocity F E, let its impulse be denoted by some aliquot part of A B, as, for instance, $\frac{4}{9}$ A B: then will $\frac{4}{9}$ of the parallelogram A F represent the mechanical power of the plane; that is, $\frac{4}{9}$ A B $\times$ $\frac{1}{3}$ B E.

2dly, Let I N be the section of a plane, inclined in such a manner, that the base I K of the rectangled triangle I K N may be equal to A B; and the perpendicular N K = B E; let the plane I N be struck by the wind, in the direction L M, perpendicular to I K; then, according to the known rules of oblique forces, the impulse of the wind upon the plane I N, tending to move it according to the direction L M, or N K, will be denoted by the base I K; and that part of the impulse, tending to move it according to the direction I K, will be expressed

by the perpendicular N K. Let the plane I N be movable in the direction of I K only; that is, the point I in the direction of I K, and the point N in the direction N Q, parallel thereto. Now, it is evident, that if the point I moves through the line I K, while a particle of air, setting forwards at the same time from the point N, moves through the line N K, they will both arrive at the point K at the same time; and, consequently, in this case also, there can be no pressure or impulse of the particle of the air upon the plane I N. Now, let I O be to I K as B F to B E; and let the plane I N move at such a rate, that the point I may arrive at O, and acquire the position I Q, in the same time that a particle of wind would move through the space N K: as O Q is parallel to I N; (by the properties of similar triangles) it will cut N K in the point P, in such a manner, that N P = B F, and P K = F E; hence, it appears that the plane I N, by acquiring the position O Q, withdraws itself from the action of the wind, by the same space N P, that the plane A B does by acquiring the position F G; and, consequently, from the equality of P K to F E, the relative impulse of the wind P K, upon the plane O Q, will be equal to the relative impulse of the wind F E upon the plane F G: and since the impulse of the wind upon A B, with the relative velocity F E, in the direction B E, is represented by $\frac{4}{9}$ A B; the relative impulse of the wind upon the plane I N, in the direction N K, will, in like manner, be represented by $\frac{4}{9}$ I K; and the impulse of the wind upon the plane I N, with the relative velocity P K, in the direction I K, will be represented by $\frac{4}{9}$ N K; and, consequently, the mechanical power of the plane I N, in the direction I K, will be $\frac{4}{9}$ the parallelogram I Q: that is $\frac{1}{3}$ I K $\times$ $\frac{4}{9}$ N K: that is, from the equality of I K = A B and N K = B E, we shall have $\frac{4}{9}$ I Q = $\frac{1}{3}$ A B $\times$ $\frac{4}{9}$ B E = $\frac{4}{9}$ A B $\times$ $\frac{1}{3}$ B E = $\frac{4}{9}$ of the area of the parallelogram A F. Hence we deduce this

GENERAL PROPOSITION.

That all planes, however situated, that intercept the same section of the wind, and having the same relative velocity, in regard to the wind, when reduced into the same direction, have equal powers to produce mechanical effects.

For what is lost by the obliquity of the impulse is gained by the velocity of the motion.

Hence, it appears that an oblique sail is under no disadvantage in respect of power, compared with a direct one; except what arises from a diminution of its breadth, in respect to the section of the wind: the breadth I N being by obliquity reduced to I K.

The disadvantage of horizontal windmills, therefore, does not consist in this,—that each sail, when directly exposed to the wind, is capable of a less power than an oblique one of the same dimensions; but that, in a horizontal windmill, little more than one sail can be acting at once: whereas, in the common windmill, all the four act together: and, therefore, supposing each vane of a horizontal windmill, of the same dimensions as each vane of the vertical, it is manifest the power of a vertical mill with four sails will be four times greater than the power of the horizontal one, let its number of vanes be what it will: this disadvantage arises from the nature of the thing: but if we consider the further disadvantage, that arises from the difficulty of getting the sails back again against the wind, &c., we need not wonder if this kind of mill is, in reality, found to have not above $\frac{1}{8}$ or $\frac{1}{10}$ of the power of the common sort; as has appeared in some attempts of this kind.

In like manner, as little improvement is to be expected from watermills with oblique vanes; for the power of the same

section of a stream of water is not greater when acting upon an oblique vane than when acting upon a direct one: and any advantage that can be made by intercepting a greater section, which sometimes may be done in the case of an open mover will be counterbalanced by the superior resistance that such vanes would meet with in moving at right angles to the current; whereas the common fanns always move with the wind nearly in the same direction.

Here it may reasonably be asked, that since our geometrical demonstration is general, and proves that one angle of obliquity is as good as another, why in our experiments it appears that there is a certain angle which is to be preferred to all the rest? It is to be observed, that if the breadth of the sail I N is given, the greater the angle K I N, and the less will be the base I K: that is, the section of wind intersected, will be less: on the other hand, the more acute the angle K I N, the less will be the perpendicular K N: that is, the impulse of the wind in the direction I K, being less, and the velocity of the sail greater; the resistance of the medium will be greater also. Hence, therefore, as there is a diminution of the section of the wind intercepted on one hand, and an increase of resistance on the other, there is some angle where the disadvantage arising from these causes, upon the whole, is the least of all: but as the disadvantage arising from resistance is more of a physical than geometrical consideration, the true angle will best be assigned by experiment.

SCHOLIUM.

In trying the experiments contained in Tab. III. and IV., the different specific gravity of the air, which is undoubtedly different at different times, will cause a difference in the load, proportional to the difference of its specific gravity, though its velocity remains the same; and a variation of specific gravity may arise not only from a variation of the weight of the whole

column, but also by the difference of heat of the air concerned in the experiment, and possibly of other causes; yet the irregularities that might arise from a difference of specific gravity were thought to be too small to be perceivable, till after the principal experiments were made, and their effects compared; from which, as well as succeeding experiments, those variations were found to be capable of producing a sensible, though no very considerable, effect: however, as all the experiments were tried in the summer season, in the day time, and under cover, we may suppose that the principal source of error would arise from the different weight of the column of the atmosphere at different times; but as this seldom varies above $\frac{1}{15}$ part of the whole, we may conclude, that though many of the irregularities contained in the experiments referred to in the foregoing essay might arise from this cause, yet, as all the principal conclusions are drawn from the medium of a considerable number, many whereof were made at different times, it is presumed that they will nearly agree with the truth, and be altogether sufficient for regulating the practical construction of those kind of machines, for which use they were principally intended.

AN

EXPERIMENTAL EXAMINATION

OF THE

QUANTITY AND PROPORTION

OF

MECHANIC POWER

NECESSARY TO BE EMPLOYED IN GIVING DIFFERENT DEGREES OF VELOCITY TO

HEAVY BODIES

FROM A

STATE OF REST.

By Mr. JOHN SMEATON, F.R.S.

AN

EXPERIMENTAL EXAMINATION, &c.

Read before the Royal Society, April 25, 1776.

About the year 1686 Sir Isaac Newton first published his *Principia,* and, conformably to the language of mathematicians of those times, defined, that "the quantity of motion is the measure of the same, arising from the velocity and quantity of matter conjointly." Very soon after this publication, the truth or propriety of this definition was disputed by certain philosophers, who contended that the measure of the quantity of motion should be estimated by taking the quantity of matter and the square of the velocity conjointly. There is nothing more certain, than that from equal impelling powers, acting for equal intervals of time, equal increases of velocity are acquired by given bodies, when unresisted by a medium; thus, gravity causes a body, in obeying its impulse during one second of time, to acquire a velocity which would carry it uniformly forward, without any additional impulse, at the rate of 32 ft. 2 in. per second; and, if gravity is suffered to act upon it for two seconds, it will have, in that time, acquired a velocity that would carry it, at an uniform rate, just double of

the former; that is, at the rate of 64 ft. 4 in. per second. Now, if, in consequence of this equal increase of velocity, in an equal increase of time, by the continuance of the same impelling power, we define that to be a double quantity of motion, which is generated in a given quantity of matter, by the action of the same impelling power for a double time,—this will be coincident with Sir Isaac Newton's definition above mentioned; whereas, in trying experiments upon the total effects of bodies in motion, it appears, that when a body is put in motion, by whatever cause, the impression it will make upon an uniformly resisting medium, or upon uniformly yielding substances, will be as the mass of matter of the moving body, multiplied by the square of its velocity: the question, therefore, properly is, whether those terms, the *quantity of motion*, the *momenta* of bodies in motion, or *forces* of bodies in motion, which have generally been esteemed synonymous, are, with the most propriety of language, to be esteemed equal, double, or triple, when they have been generated by an equable impulse, acting for an equal, double, or triple time; or that it should be measured by the effects being equal, double, or triple, in overcoming resistances before a body in motion can be stopped? For, according as those terms are understood in this or that way, it will necessarily follow, that the *momenta* of equal bodies will be as the velocities, or as the squares of the velocities respectively; it being certain that, whichever we take for the proper definition of the term quantity of motion, by paying a proper regard to the collateral circumstances that attend the application of it, the same conclusion, in point of computation, will result. I should not, therefore, have thought it worth while to trouble the Society upon this subject, had I not found, that not only myself and other practical artists, but also some of the most approved writers, had been liable to fall into errors, in applying these doctrines to practical mechanics, by sometimes forgetting or neglecting the due regard which ought to be had to these collateral circumstances. Some of these errors are not only very

considerable in themselves, but also of great consequence to the public, as they tend greatly to mislead the practical artist in works that occur daily, and which often require very great sums of money in their execution. I shall mention the following instances.

Desaguliers, in his second volume of Experimental Philosophy, treating upon the question concerning the forces of bodies in motion, after taking much pains to shew that the dispute, which had then subsisted fifty years, was a dispute about the meaning of words; and that the same conclusion will be brought out, when things are rightly understood, either upon the old or new opinion, as he distinguishes them; among other things, tells us, that the old and new opinion may be easily reconciled in this instance; that the wheel of an undershot watermill is capable of doing quadruple work when the velocity of the water is doubled, instead of double work only; "because, the adjutage being the same," says he, "we find, that as the water's velocity is double, there are twice the number of particles of water that issue out, and, therefore, the ladle-board is struck by twice the matter, which matter moving with twice the velocity that it had in the first case, the whole effect must be quadruple, though the instantaneous stroke of each particle is increased only in a simple proportion of the velocity." See Vol. II. Annotations on Lecture 6th, p. 92.

Again, in the same volume, lecture 12th, p. 424, referring to what went before, he tells us, "The knowledge of the foregoing particulars is absolutely necessary for setting an undershot wheel to work; but the advantage to be reaped from it would be still guess work, and we should be still at a loss to find out the utmost it can perform, if we had not an ingenious proposition of that excellent mechanic, M. Parent, of the Royal Academy of Sciences, who has given us a *maximum* in this case, by shewing that an undershot wheel can do the most work, when its velocity is equal to the third part of the

velocity of the water that drives it, &c., because then two-thirds of the water is employed in driving the wheel with a force proportionable to the square of its velocity. If we multiply the surface of the adjutage, or opening, by the height of the water, we shall have the column of water that moves the wheel. The wheel, thus moved, will sustain on the opposite side only four-ninths of that weight, which will keep it in equilibrio; but what it can move with the velocity it goes with, will be but one-third of that weight of equilibrium; that is, $\frac{4}{27}$ths of the weight of the first column, &c. This is the utmost that can be expected."

The same conclusion is likewise adopted by Maclaurin, in art. 907, p. 728 of his Fluxions, where, giving the fluxionary deduction of M. Parent's proposition, he says, "that if A represents the weight which would balance the force of the stream, when its velocity is a; and U represents the velocity of the part of the engine, which it strikes when the motion of the machine is uniform, &c. The machine will have the greatest effect when U is equal to $\frac{a}{3}$; that is, if the weight that is raised by the engine be less than the weight which would balance the power, in the proportion of 4 to 9, and the *momentum* of the weight is $\frac{4Aa}{27}$."

Finding that these conclusions were far from the truth, and seeing, from many other circumstances, that the practical theory of making water and windmills was but very imperfectly delivered by any author I had then an opportunity of consulting;*

* Belidor, *Architecture Hydraulique*, greatly prefers the application of water to an undershot mill, instead of an overshot; and attempts to demonstrate, that water applied undershot, will do six times more execution than the same applied overshot. See Vol. I. p. 186. While Desaguliers, endeavouring to invalidate what had been advanced by Belidor, and greatly preferring an overshot to an undershot, says (Annotat. on Lecture 12, Vol. II. p. 532), that from his own expe-

in the year 1751 I began a course of experiments upon this subject. These experiments, with the conclusions drawn from them, have already been communicated to this Society, who printed them in Vol. LI. of their Transactions for the year 1759, and for this communication I had the honour of receiving the annual medal of Sir Godfrey Copley, from the hands of our very worthy President, the late Earl of Macclesfield. These experiments and conclusions stand uncontroverted, so far as I know, to this day: and having since that time been concerned in directing the construction of a great number of mills, which were all executed upon the principles deduced from them, I have by that means had many opportunities of comparing the effects actually produced with the effects which might be expected from the calculation; and the agreement I have always found between these two, appears to me fully to establish the truth of the principles upon which they were constructed, when applied to great works, as well as upon a smaller scale in models.

Respecting the explanatory deduction of Desaguliers in the first example above mentioned, which, indeed, I have found to be the commonly received doctrine among theoretical mechanics, it is shewn, in my former Essay, pp. 24, 25, and 27, Part I., Maxim 4, that, where the velocity of water is double, the adjutage or aperture of the sluice remaining the same, the effect is eight times; that is not as the square, but as the cube of the velocity; and the same is investigated concerning the

rience, " a well-made overshot mill ground as much corn in the same time with ten times less water; so that betwixt Belidor and Desaguliers, here is a difference of no less than 60 to 1.

Again, Belidor, Vol. II. p. 72, says, that the centre of gravity of each sail of a windmill should travel in its own circle with one-third of the velocity of the wind; so that, taking the distance of this centre of gravity from the centre of motion at 20 feet, as he states it, p. 38, art. 849, the circumference will be exceeding 126 feet English measure: a wind, therefore, to make the mill go twenty turns per minute, which they frequently do with a fresh wind, and all their cloth spread, would require the wind to move above eighty miles an hour; a velocity perhaps hardly equalled in the greatest storms we experience in this climate.

power of the wind arising from difference of velocity, p. 62, being Part III., Maxim 4.

The conclusion in the second example above-mentioned, adopted both by Desaguliers and Maclaurin, is not less wide of the truth than the foregoing; for, if that conclusion were true, only $\frac{4}{27}$ths of the water expended could be raised back again to the height of the reservoir from which it had descended, exclusively of all kinds of friction, &c., which would make the actual quantity raised back again still less; that is, less than one-seventh of the whole; whereas, it appears from Tab. I. of the preceding Essay, that in some of the experiments there related, even upon the small scale on which they were tried, the work done was equivalent to raising back again about one quarter of the water expended: and in large works the effect is still greater, approaching towards half, which seems to be the limit for the undershot mills, as the whole would be the limit for the overshot mills, if it were possible to set aside all friction, resistance from the air, &c. See p. 37.

The velocity also of the wheel, which, according to M. Parent's determination, adopted by Desaguliers and Maclaurin, ought to be no more than one-third of that of the water, varies at the *maximum* in the above mentioned experiments of Tab. I., between one-third and one-half; but in all the cases there related, in which the most work is performed in proportion to the water expended, and which approach the nearest to the circumstances of great works, when properly executed, the *maximum* lies much nearer to one-half than one-third; one-half seeming to be the true *maximum*, if nothing were lost by the resistance of the air, the scattering of the water carried up by the wheel, and thrown off by the centrifugal force, &c., all which tend to diminish the effect more, at what would be the *maximum* if these did not take place than they do when the motion is a little slower.

Finding these matters, as well as others, to come out in the experiments so very different from the opinions and calculations of authors of the first reputation, who, reasoning according to the Newtonian definition, must have been led into these errors from a want of attending to the proper collateral circumstances; I thought it very material, especially for the practical artist, that he should make use of a kind of reasoning in which he should not be so liable to mistakes; in order, therefore, to make this matter perfectly clear to myself, and possibly so to others, I resolved to try a set of experiments, from whence it might be inferred what proportion or quantity of mechanical power is expended in giving the same body different degrees of velocity.* This scheme was put in execution in the year 1759, and the experiments were then shewn to several friends, particularly my very worthy and ingenious friend, Mr. William Russel.

In my Experimental Inquiry concerning the Powers of Water and Wind, before referred to, I have, p. 6, Part I. defined what I meant by power as applied to practical mechanics, that is, what I now call mechanical power; which, in

* The charge of these mistakes to reasoning on Newton's definition of the measure of motion is not quite correct. Desaguliers reasons correctly as far as his reasoning extends, in p. 77. And the force on the paddle or float-boards will be in the ratio he has assigned; but the velocity of the wheel must be increased so as to bear the same relation to the velocity of the stream as before, and, therefore, the force on the wheel will be as the square, and the work done, as the cube of the velocity. If Desaguliers did not intend so, he contradicts himself in the next paragraph, when he states that the column of water drives "the wheel with a force proportionable to the square of the velocity," and with a velocity equal to a third part of the velocity of the water.

The defect of the investigations of Parent and Maclaurin is quite of a different nature. They suppose the impulse of a fluid to be proportional to the square of the difference between the velocities of the stream and the wheel. This appears to be true only when a float-board impels a fluid; and the right proportion when the fluid impels the wheel is, the difference between the squares of the velocities of the stream before and after its action on the wheel. The application of this theory is shewn in the note on p. 14.—Ed.

terms synonymous to those there used, may be said to be measured by multiplying the weight of the body into the perpendicular height from which it can descend; thus the same weight, descending from a double height, is capable of producing a double mechanical effect, and is, therefore, a double mechanical power. A double weight descending from the same height is also a double power, because it likewise is capable of producing a double effect; and a given body, descending through a given perpendicular height, is the same power as a double body descending through half that perpendicular; for, by the intervention of proper levers, they will counterbalance one another, conformably to the known laws of mechanics, which have never been controverted. It must, however, be always understood, that the descending body, when acting as a measure of power, is supposed to descend slowly, like the weight of a clock or a jack; for, if quickly descending, it is sensibly compounded with another law, viz. the law of acceleration by gravity.

DESCRIPTION OF THE MACHINE.

A B is the base of the machine placed upon a table.

A C is a pillar or standard.

C D is an arm, upon the extremity of which is fixed a plate *f g*, which is here seen edgeways, through which is a small hole for receiving a small steel pivot *e*, fixed in the top of the upright axis *e* B; the lower end of this axis finishes in a conical steel point, which rests upon a small cup of hard steel polished at B.

H I is a cylinder of white fir, which passes through a perforation in the axis, and therein fixes; and, upon the two arms formed thereby, are capable of sliding.

K L, two cylindric weights of lead of equal size, which are capable of being fixed upon any part of the cylindric arms, from the axis to their extremities, by means of two thin wedges of wood. The two weights, therefore, being at equal distances from the centre, and the axis perpendicular, the whole will be balanced upon the point B, and movable thereupon by an impelling power, with very little friction.

Upon the upper part of the axis are formed M N, two cylindrical barrels, whereof M is double the diameter of N; they have a little pin stuck into one side of each at *o*, *p*.

Q is a piece capable of sliding higher or lower, as occasion requires; and carries

R, a light pulley of about three inches diameter, hung upon a steel axis, and movable upon two small pivots. The plane of the pulley, however, is not directed to the middle of the upright axis, but a little on one side, so as to point (at a mean) between the surface of the bigger barrel and the less.

S is a light scale for receiving weights, and hangs by a small twine, cord, or line, that passes the pulley, and terminates either upon the bigger barrel or the less, as may be required; the sliding piece Q being moved higher or lower for each, that the line, in passing from the pulley to the barrel, may be nearly horizontal. The end of the line that is furthest from the scale is terminated by a small loop, which hangs upon the pin *o*, or the pin *p*, according as the bigger or the lesser barrel is to be used.

Now, having wound up a certain number of turns of the line upon the barrel, and having placed a weight in the scale S, it is obvious, that it will cause the axis to turn round, and give motion to its arms, and to the weights of lead placed thereon, which are the heavy bodies to be put in motion by the impulse

of the weight in the scale; and when the line is wound off to the pin, the loop slips off, and the scale then falling down, the weight will cease to accelerate the motion of the heavy bodies, and leave them revolving, equably forward, with the velocity they have acquired, except so far as it must be gradually lessened by the friction of the machine and resistance of the air, which being small, the bodies will revolve sometime before their velocity is apparently diminished.

MEASURES OF SOME PARTS OF THE MACHINE.

	Inches.
Diameter of the cylinders of lead or the heavy bodies ..	2·57
Length of ditto	1·56
Diameter of the hole therein	·72

Weight of each cylinder 3 lbs. avoirdupois.

Greater distance of the middle of each body from the centre of the axis	8·25
The smaller distance of ditto.....	3·92
10 turns of the smaller barrel raises the scale, 5 ditto of the bigger ditto	25·25

When the bodies are at the smaller distance above specified, from the axis of rotation, they are then in effect at half the greater distance from that axis: for, since the axis itself, and the cylindric arms of wood, keep an unvaried distance from the centre of rotation, the bodies themselves must be moved nearer than half their former distance, in order that, compounded with the unvariable parts, they may be virtually at the half distance. In order to find this half distance nearly, I put in an arm of the same wood, that only went through the axis, without extending in the opposite direction; one of the bodies being put upon the end of this arm, at the distance of 8·25 inches, the whole machine was inclined till the body and arm became a kind of pen-

dulum, and vibrated at the rate of 92 times per minute; and as a pendulum of the half length vibrates quicker in the proportion of $\sqrt{1}$ to $\sqrt{2}$; that is, in the proportion of 92 to 130 nearly; therefore, keeping the same inclination of the machine, the weight was moved upon the arm till it made 130 vibrations per minute; which was found to be, when it was at 3·92 inches distance from the centre, as above stated, which is about $\frac{2}{10}$ths of an inch nearer than the half distance. The double arm was then put in, and marked accordingly, and the bodies being mounted thereon, the whole was adjusted ready for use; and with it were tried the following experiments, each of which was repeated so many times as to be fully satisfactory.

TABLE OF EXPERIMENTS.

No.	Ounces avoirdupois in the Scale.	Barrel used, M the bigger, N the smaller.	The arms, W the whole, H the half length.	Number of turns of the line wound on the Barrel.	Time of the Descent of the Weight in the Scale.	Time in making 20 Revolutions with equable motions.
1	8	M	W	5	14¼″	29′
2	8	N	W	10	28½″	29¼′
3	8	N	W	2½	14¼″	58½′
4	32	M	W	5	7	14
5	32	N	W	10	14	14¾
6	32	N	W	2½	7	28¾
7	8	M	H	5	7	14¾
8	8	N	H	10	14	15
9	8	N	H	2½	7	30¼
1	2	3	4	5	6	7

The $58\frac{1}{2}''$ in No. 3, col. 7, was determined in fact from $29\frac{1}{4}''$, being the time of making 10 equable revolutions after the weight was dropped off, in order to prevent the sensible retardation that might take place and affect the observation, if continued for 20 revolutions made so slowly.

FURTHER DEFINITIONS.

I have already defined what I mean by mechanic power; but, before I proceed further, it will be necessary also to define the following terms:—

Impulse or Impulsion,
Impulsive Force or Power,
Impelling Force or Power,
} By all which, I understand the uniform endeavour that one body exerts upon another, in order to make it move; and that, whether it produces or generates motion by this endeavour or not; and the quantity of this impelling power may be measured either by its being a weight of itself, or by being counterbalanced by a weight. It may also act either immediately upon the body to be moved, so that if motion is the consequence, they move with the same velocity, and that, either by a simple contact, or by being drawn as by a cord, or pushed as by a staff; or it may act by the intervention of a lever or other mechanic instrument, in which the velocity of the body to be moved may be very different from the velocity of the impelling power or mover: but, in comparing them, the impelling powers must be reduced according to the proportional velocities of the mover and moved; or, in levers of different lengths, they may be compared by a standard length of lever, which is the method taken in the subsequent reasoning upon the preceding experiments. An impelling power, therefore, consisting of a double weight, or requiring a double weight to counterbalance it, when acting with equal levers, is a double impelling power, or an impelling power of double the intensity.

OBSERVATIONS AND DEDUCTIONS FROM THE PRECEDING EXPERIMENTS.

1st. By the first experiment it appears, that the mechanic power employed, consisting of eight ounces in the scale, deliberately descending (by 5 turns of the bigger barrel) through a perpendicular space 25¼ inches, will represent the quantity of mechanic power which causes the two heavy bodies, from a state of rest, to acquire a velocity such as to carry them equably through 20 circumferences of their circle of revolution in the space of 29″; and that the time in which the mechanic power produced this effect was 14¼″, as appears by column 6th. And this mechanic power we shall express by the number 202, the product of the number of ounces in the scale multiplied by the inches in its perpendicular descent, for $8 \times 25\frac{1}{4} = 202$.

2dly. By the second experiment, as 10 turns of the smaller barrel are equal to the same perpendicular height as 5 turns of the bigger, it follows that the same mechanic power, viz. 202, acting upon the same heavy bodies to accelerate them, produces the very same effect in generating motion in the bodies as it did before, viz. 30 revolutions in 29¼″, the small difference of ¼ of a second being no more than may reasonably be attributed to the unavoidable errors arising from friction of the machine, want of perfect accuracy in its measures, resistance of the air, and imperfections in the observations themselves, which must not only be allowed for in this, but the rest; but as the impelling power is acting here upon a lever of but half the length, and, consequently, but half the intensity, when referred to the bodies to be moved, it takes just double the time to generate the same velocity therein.

Deduction. It appears from hence, that the same mechanic power is capable of producing the same velocity in a given

body, whether it is applied so as to produce it in a greater or a lesser time; but that the time taken to produce a given velocity by an uniformily continued action, is in a simple inverse proportion of the intensity of the impulsive power.

3dly. The third experiment being made with $2\frac{1}{2}$ turns of the lesser barrel, the same weight in the scale of 8 ounces descending only one quarter part of the former perpendicular, the mechanic power employed will be only one quarter part of the former, viz. $50\frac{1}{2}$; but, as one quarter part of the mechanic power produces half of the former velocity in the heavy bodies—that is, they make 20 revolutions in $58\frac{1}{2}''$, that is near 10 revolutions in $29''$—we may conclude, in this instance, that the mechanic power employed in producing motion, is as the square of the velocity produced in the same body: and that the velocity produced, is as the time that an impelling power of the same intensity continues to act upon it, as appears by the near agreement of Nos. 2 and 3, col. 6th.

4thly. In the fourth experiment, the apparatus is the same as the first, only here the weight in the scale is 32 ounces; that is, the impelling power is the quadruple of the first, and hereby a double velocity is given to the bodies, for they make 20 revolutions in $14''$, which is a small matter less than half the time taken up in making 20 revolutions in the first experiment. It also appears, that the velocity acquired is simply as the impelling power compounded with the time of its action; for a quadruple impulsion, acting for $7''$ instead of $14''$, generates a double velocity, while the mechanic power employed to generate it is quadruple, for $32 \times 25\frac{1}{4} = 808$. And here the mechanic power employed being four times greater than the first, it holds here also, that the mechanic power, to be necessarily employed, is as the square of the velocity to be generated; that is, in the same proportion as turned out in the third experiment, where the mechanic power employed was only a quarter part of the first.

5thly. The fifth and sixth experiments were made with a mechanic power four times greater than those employed in Nos. 2 and 3 respectively; and since the same deductions result from hence as from Nos. 2 and 3, they are additional confirmations of the conclusions drawn from them, and from the last article.

6thly. In the seventh experiment, the disposition of the apparatus is the same as No. 1, only here the bodies are placed upon the arms at the half-length; from whence it appears, that the same mechanic power still produces the same velocity in the same bodies; for, though 20 revolutions were performed in $14\frac{3}{4}''$ (see col. 7), which is nearly half the time that 20 revolutions were performed in the first experiment; yet, since the circles in which the bodies revolved in the seventh are only of half the circumference as those of No. 1, it is obvious, that the absolute velocity acquired by the moving bodies in the two cases is equal. But, by col. 6th, the time in which it was generated is only half; yet, notwithstanding, this will coincide with the former conclusions, if the intensity of the impelling power is compounded therewith; for, though the barrel was the same, with the same number of turns, as in No. 1, and, therefore, the lever the same, by which the impelling power acted; yet, as the bodies upon which this lever was to act, were placed upon a lever of only half the length from the centre, the impelling power acting by the first lever, would act upon the second with double the intensity, according to the known laws of mechanics; that is, it would require a double weight, opposing the bodies, to prevent their moving, in order to balance it. An impulsive power, therefore, of double the intensity, acting for half the time, produces the same effect in generating motion, as an impulsive power, of half the intensity acting for the whole time.

7thly. The eighth and ninth experiments afford the same deductions and confirmations relative to the seventh experiment,

that the fifth and sixth do respecting the fourth, and that the second and third do respecting the first; and from the near agreement of the whole, when the necessary allowances beforementioned are made, together with some small inequality arising from the mechanical power lost by the difference of the motion given by gravity to the weight in the scale: I say, from these agreements under the very different mechanical powers applied, which were varied in the proportion of 1 to 16, we may safely conclude, that this is the universal law of nature respecting the capacities of bodies in motion to produce mechanical effects, and the quantity of mechanic power necessary to be employed to produce or generate different velocities (the bodies being supposed equal in their quantity of matter); that the mechanic powers to be expended, are as the squares of the velocities to be generated, and *vice versâ*; and that the simple velocities generated are as the impelling power compounded with, or multiplied by, the time of its action, and *vice versâ*.

We shall, perhaps, form a still clearer conception of the relation between velocities produced and the quantity of mechanic power required to produce them, together with the collateral circumstances attending, by which these propositions, seemingly two, are reconciled and united, by stating the following popular elucidation, which, indeed, was the original idea that occurred to me on considering this subject; to put which to an experimental proof, gave birth to the foregoing apparatus and experiments.

Suppose, then, a large iron ball of 10 feet diameter, turned truly spherical, and set upon an extended plane of the same metal, and truly level. Now, if a man begins to push at it, he will find it very resisting to motion at first; but by continuing the impulse, he will gradually get it into motion, and having nothing to resist it but the air, will, by continuing his efforts, at length get it to roll almost as fast as he can run.

Suppose, now, in the first minute, he gets it rolled through a space of one yard; by this motion, proceeding from rest (similar to what happens to falling bodies), it would continue to roll forward, at the rate of two yards per minute, without further help; but supposing him to continue his endeavours, at the end of another minute he will have given it a velocity capable of carrying it through a space of two yards more, in addition to the former, that is, at the rate of four yards per minute; and at the end of the third minute, he has again added an equal increase of velocity, and made it proceed at the rate of six yards per minute; and so on, increasing its velocity at the rate of two yards in every minute. The man, therefore, in the space of every minute exerts an equal impulse upon the ball, and generates an equal increase of movement correspondent to the definition of Sir Isaac Newton. But let us see what happens besides: the man, in the first minute, has moved but one yard from where he set out; but he must, in the second minute, move two yards more, in order to keep up with the ball; and as he exerted an impulse upon it, so as at the end of the second minute to have given it an additional velocity of the two yards, he must, also, in this time have gradually changed its velocity from the rate of two yards per minute to that of four, and the space, that he will of consequence have actually been obliged to go through in the second minute, will be according to the mean of the extremes of velocity at the beginning and end thereof, that is, three yards in the second minute; so that, being one yard from its original place at the beginning of the second minute, at the end of it he will have moved the sum of the journeys of the first and second minute, that is in the whole four yards from his original place. As he has now generated a velocity in the ball of four yards per minute, in the third minute he must travel four yards to keep up with the ball, and one more in generating the equal increment of velocity; so that, in the third minute, he must travel five yards to keep up the same impelling power upon the ball that he did in the first minute in

travelling one, so that these five yards, in the third minute, added to the four yards that he had travelled in the two preceding minutes, sets him at the end of the third minute, nine yards from whence he set out, having then given the ball a velocity capable of carrying it uniformily forward at the rate of six yards per minute, as before stated. We may now leave the further pursuit of these proportions, and see how the account stands. He generated a velocity of two yards per minute in the first minute, the square of which is four, when he had moved but one yard from his place; and he had generated a velocity of six yards per minute, the square of which is thirty-six, at the end of the third minute, when he had travelled nine yards from his place. Now, since the square of the velocity, generated at the end of the first minute, is to that of the velocity generated at the end of the third minute, as 4 : 36, that is as 1 : 9; and since the spaces moved through by the man to communicate these velocities, are also as 1 : 9, it follows, that the spaces through which the man must travel, in order to generate these velocities respectively (preserving the impelling power perfectly equal) must be as the squares of the velocities that are communicated to the ball; for, if the man was to be brought back again to his original place by a mechanical power, equally exerted upon the man equally resisting, this would be the measure of what the man has done in order to give motion to the ball. It, therefore, directly follows, conformably to what has been deduced from the experiments, that the mechanic power that must of necessity be employed in giving different degrees of velocity to the same body, must be as the square of that velocity; and if the converse of this proposition did not hold, viz. that if a body in motion, in being stopped, would not produce a mechanical effect, equal or proportional to the square of its velocity, or to the mechanical power employed in producing it, the effect would not correspond with its producing cause.

Thus, the consequences of generating motion upon a level plane exactly correspond with the generating of motion by gravity; viz. that, though in two seconds of time, the equal impulsive power of gravity gives twice the velocity to a body that it does in one second, yet this collateral circumstance attends it, that, at the end of the double time, in consequence of the velocity acquired in the first half the body has fallen from where it set forward through four times the perpendicular; and, therefore, though the velocity is only doubled, yet four times the mechanical power had been consumed in producing it, as four times the mechanical power must be expended in bringing up the fallen body to its first place.

This, then, appears to be the foundation, not only of the disputes that have arisen, but of the mistakes that have been made, in the application of the different definitions of quantity of motion; that while those, that have adhered to the definition of Sir Isaac Newton, have complained of their adversaries, in not considering the time in which effects are produced, they themselves have not always taken into the account the space that the impelling power is obliged to travel through in producing the different degrees of velocity. It seems, therefore, that, without taking in the collateral circumstances, both of time and space, the terms, quantity of motion, *momentum*, and force of bodies in motion, are absolutely indefinite; and that they cannot be so easily, distinctly, and fundamentally compared, as by having recourse to the common measure, viz. mechanic power.

From the whole of what has been investigated, it therefore appears, that time, properly speaking, has nothing to do with the production of mechanical effects, otherwise than as, by equally flowing, it becomes a common measure; so that, whatever mechanical effect is found to be produced in a given time, the uniform continuance of the action of the same me-

chanical power will, in a double time, produce two such effects, or twice that effect. A mechanical power, therefore, properly speaking, is measured by the whole of its mechanical effect produced, whether that effect is produced in a greater or a lesser time; thus, having treasured up 1000 tuns of water, which I can let out upon the overshot wheel of a mill, and, descending through a perpendicular of 20 feet, this power applied to proper mechanic instruments, will produce a certain effect, that is, it will grind a certain quantity of corn: and that, at a certain rate of expending it, it will grind this corn in an hour. But suppose the mill equally adapted to produce a proportionable effect, by the application of a greater impulsive power as with a less; then, if I let out the water twice as fast upon the wheel, it will grind the corn twice as fast, and both the water will be expended, and the corn ground in half an hour. Here the same mechanical effect is produced; viz. the grinding a given quantity of corn, by the same mechanical power, viz. 1000 tuns of water, descending through a given perpendicular of 20 feet, and yet this effect is in one case produced in half the time of the other. What time, therefore, has to do in the business is this: let the rate of doing the business, or producing the effect, be what it will, if this rate is uniform, when I have found by experiment what is done in a given time, then proceeding at the same rate, twice the effect will be produced in twice the time, on supposition that I have a supply of mechanic power to go on with. Thus, 1000 tuns of water, descending through 20 feet of perpendicular, being, as has been shewn, a given mechanic power, let me expend it at what rate I will, if, when this is expended, I must wait another hour before it be renewed, by the natural flow of a river, or otherwise, I can then only expend twelve such quantities of power in 24 hours: but if, while I am expending 1000 tuns in one hour, the stream renews me the same quantity, then I can expend 24 such quantities of power in 24 hours: that is, I can go on continually at that rate, and the product or effect

will be in proportion to time, which is the common measure; but the quantity of mechanic power arising from the flow of the two rivers, compared by taking an equal portion of time, is double in the one to the other, though each has a mill, that, when going, will grind an equal quantity of corn in an hour.

NEW

FUNDAMENTAL EXPERIMENTS

UPON THE

COLLISION OF BODIES.

By Mr. JOHN SMEATON, F.R.S.

[illegible]

COLLISION OF BODIES

Read before the Royal Society, [illegible]

It is universally acknowledged that the first simple principles of science cannot be too carefully examined, in order to their being firmly established: more especially those which relate to the practical and operative parts of mechanics, upon which much of the active business of mankind depends. A consideration of this kind occasioned my tract upon *Motion*, which is the second paper of this volume. What I have now to offer was intended as a supplement thereto, and the experiments were then, in part, tried: and the completion thereof was deferred at that time, partly from want of leisure; partly to avoid too great a length of the paper itself: and partly to avoid the bringing forward too many points at once.

My present purpose is to shew, that the true doctrine of the *Collision of Bodies* hangs, as it were, upon the same hook as the doctrine of the gradual generation of motion from rest, considered in that paper; that is, that whether bodies are put into

gradual motion, and uniformly accelerated from rest to any given velocity; or are put in motion in an instantaneous manner, when bodies of any kind strike one another; the motion or sum of the motions produced, has the same relation to mechanic power therein defined, which is necessary to produce the motion desired. To prove this, and at the same time to shew some capital mistakes in principle, which have been *assumed* as indisputable truths by men of great learning, is the reason of my now pursuing the same subject.

I do not mean to point out the particular mistakes which have been made by particular men, as that would lead me into too great a length; I shall, therefore, content myself with observing, that the laws of collision, which have been investigated by mathematical philosophers, are principally of three kinds; viz. those relating to bodies perfectly *elastic;* to bodies perfectly unelastic, and perfectly *soft;* and to bodies perfectly unelastic, and perfectly *hard.* To avoid prolixity, I shall consider in each, only the simple case of two bodies which are equal in weight or quantity of matter striking one another. Respecting those which are perfectly elastic, it is universally agreed, that when two such bodies strike one another, no motion is lost; but that in all cases, what is lost by one is acquired by the other: and hence, that if an elastic body in motion strikes another at rest, upon the stroke the former will be reduced to a state of rest, and the latter will fly off with an equal velocity.

In like manner, if a non-elastic *soft* body strikes another at rest, they neither of them remain at rest, but proceed together from the point of collision with exactly one half of the velocity that the first had before the stroke: this is also universally allowed to be true, and is fully proved by every good experiment upon the subject.

Respecting the third species of body, that is, those that are non-elastic, and yet perfectly hard: the laws of motion relating

to them, as laid down by one species of philosophers, have been rejected by another; the latter alleging that there are no such bodies to be found in nature whereon to try the experiment; but those who have laid down and assigned the doctrine that would attend the collision of bodies of this kind (if they could be found), have universally agreed, that if a non-elastic *hard* body was to strike another of the same kind at rest, that, in the same manner as is agreed concerning non-elastic soft bodies, they neither of them would remain at rest, but would in like manner proceed from the point of collision, with exactly one-half of the velocity that the first had before the stroke: in short, they lay it down as a rule attending all non-elastic bodies, whether hard or soft, that the velocity after the stroke will be the same in both, viz. *one-half* of the velocity of the original striking body.

Here is, therefore, the assumption of a principle, which, in reality is proved by no experiment, nor by any fair deduction of reason that I know of, viz. that the velocity of non-elastic *hard* bodies after the stroke must be the same as that resulting from the stroke of non-elastic *soft* bodies; and the question now is, whether it be true or not?

Here it may be very properly asked, what ill effects can result to practical men, if philosophers should reason wrong concerning the effects of what does not exist in nature, since the practical men can have no such materials to work upon, or misjudge of? But it is answered, that they who infer an equality of effects between the two sorts, may, from thence, be misled themselves, and, in consequence, mislead practical men in their reasonings and conclusions concerning the sort with which they have abundant concern, to wit, the non-elastic *soft bodies*, of which water is one, which they have much to do with in their daily practice.

Previous to the trying my experiment on mills, I never had doubted the truth of the doctrine, that the same velocity resulted

from the stroke of both sorts of non-elastic bodies; but the trial of those experiments made me clearly see at least the inconclusiveness, if not the falsity, of that doctrine: because I found a result which I did not expect to have arisen from either sort; and for the which, when it appeared from experiment, I could see a substantial reason why it should take place in one sort, and that it was impossible that it could take place in the other; for, if it did, the bodies could not have been perfectly *hard*, which would be contrary to the hypothesis: of this deduction I have given notice in my said tract on mills, p. 38.—*The effect, therefore, of overshot wheels, &c.*

It may also be said, that since we have no bodies perfectly elastic, or perfectly unelastic and *soft*, why should we expect bodies perfectly unelastic and *hard?* Why may not the effects be such as should result from a supposition of their being *imperfectly elastic* joined with their being *imperfectly hard?* But here I must observe, that the supposition appears to be a contradiction in terms.

We have bodies which are so nearly perfectly elastic, that the laws may be very well deduced and confirmed by them; and the same obtains with respect to non-elastic *soft* bodies; but, concerning bodies of a mixed nature, which are by far the greatest number, so far as they are wanting in elasticity, they *are soft*, and *bruise, yield*, or *leave a mark* in collision; and so far as they are not perfectly soft, they are elastic, and observe a mixture of the law relative to each; but imperfectly elastic bodies, imperfectly hard, come, in reality, under the *same description* as the former mixed bodies; for, so far as they are imperfectly hard, they are soft, and either *bruise* and *yield*, or leave a mark in the stroke; and so far as they want perfect elasticity, they are non-elastic; that is to say, they are bodies imperfectly elastic, and imperfectly soft; and in fact, I have never yet seen any bodies but what come under this description. It seems, therefore, that respecting the *hardness* of bodies they differ in de-

grees of it, in proportion as they have a greater degree of tenacity or cohesion; that is, are further removed from perfect softness, at the same time that their elastic springs, so far as they reach, are very stiff: and hence we may (by the way) conclude, that the same mechanic power that is required to change the figure, in a *small degree*, of those bodies that have the popular appellation of *hard bodies*, would change it in a *great degree* in those bodies that approach towards softness, by having a small degree of tenacity or cohesion. In the former kind we may rank the harder kinds of *cast iron*, and in the latter, *soft tempered clay*.

While the philosophical world was divided by the dispute about the *old* and *new opinion*, as it was called, concerning the power of bodies in motion, in proportion to their different velocities; those who held the old opinion contending that it was as the velocity *simply*, asked those of the new, how, upon their principles, they would get rid of the conclusions arising from the doctrine of unelastic perfectly hard bodies? They replied, they found no such bodies in nature, and, therefore, did not concern themselves about them. On the other hand, those of the new opinion asked those of the old, how they would account for the case of non-elastic soft bodies, where, according to them, the whole motion lost by the striking body was retained in the two after the stroke (the two bodies moving together with the half velocity), though the two non-elastic bodies had been bruised and changed their figure by the stroke; for, if no motion was lost, the change of figure must be an effect without a cause? To obviate this, those of the old opinion seriously set about proving, that the bodies might change their figures, without any loss of motion in either of the striking bodies.

Neither of these answers have appeared to me satisfactory, especially since my mill experiments: for, with respect to the first, it is no proper argument to urge the impossibility of

finding the proper material for an experiment, in answer to a conclusion drawn from an abstract idea. On the other hand, if it can be shewn that the figure of a body can be changed, without a *power*, then, by the same law, we might be able to make a *forge hammer* work upon a mass of soft iron, without any other power than that necessary to overcome the friction, resistance, and original *vis inertiæ*, of the parts of the machine to be put in motion: for, as no progressive motion is given the mass of iron by the hammer (it being supported by the anvil), no power can be expended that way; and, if none is lost to the hammer from changing the figure of the iron, which is the only effect produced, then the whole power must reside in the hammer, and it would jump back again to the place from which it fell, just in the same manner as if it fell upon a body perfectly elastic, upon which, if it did fall, the case would really happen: the power, therefore, to work the hammer would be the same, whether it fell upon an elastic or non-elastic body; an idea so very contrary to all experience, and even apprehension, of both the philosopher and the vulgar artist, that I shall here leave it to its own condemnation.

As nothing, however, is so convincing to the mind as experiments obvious to the senses, I was very desirous of contriving an experiment in point; and as I saw no hopes of finding matter to make a *direct* experiment, I turned my mind towards an indirect one: so circumscribed, however, as to prove incontestably, that the result of the stroke of two non-elastic perfectly hard bodies could not be the same as would result from the collision of two soft ones; that is, if it can be *bonâ fide* proved, that *one-half* of the original power is lost in the stroke of soft bodies by the change of figure (as was very strongly suggested by the mill experiments): then, since no such loss can happen in the collision of bodies perfectly hard, the result and consequence of such a stroke must be *different*.*

* Examining the experiments alluded to, we find that the whole mass of the

The consequence of a stroke of bodies perfectly hard, but void of elasticity, must doubtless be different from that of bodies perfectly *elastic:* for, having no spring, the body at rest could not be driven off with the velocity of the striking body, for that is the consequence of the action of the spring or elastic parts between them, as will be shewn in the result of the experiments; the striking body will, therefore, not be stopped, and as the motion it loses must be communicated to the other, from the equality of action and re-action, they will both proceed together, with an equal velocity,* as in the case of non-elastic soft bodies: the question, therefore, that remains is, what that *velocity must be?* It must be greater than that of the non-elastic soft bodies, because there is no mechanical power lost in the stroke. It must be less than that of the striking body, because, if equal, instead of a loss of motion by the collision it will be doubled.

water escapes with about half the velocity of the stream; hence, one half of the fall is employed in this part of the operation on the supposition that the effect is due to the action of an uniform force acting with an uniform velocity. The other half of the power of the stream produces the same effect as in an overshot mill; consequently, no loss by change of figure, more than of the species termed friction, occurs in the action of fluids by impulse; they, therefore, cannot be considered of the nature of soft bodies. The distinction between the generation of the motion of the water by the free action of gravity on the parts of the fluid, and the continuance of an uniform motion by the constrained action of gravity has not been preserved. The descent of a heavy body, falling freely one second, is 16 feet, and its final velocity 32 feet per second; and it is exactly an equivalent effect to that of the same body descending 16 feet with an uniform velocity of 16 feet per second. The descent of a fluid is of the same nature as that of a falling body; and hence it can act only with half its final velocity in raising an equal mass, with an uniform motion to the same height which generated its velocity. The final *momenta* in the two cases are not equal: for, in the body descending by accelerated motion, the whole power is collected in the body; in the other, it must be imparted as it is impressed.—ED.

* In order that two bodies may proceed together, the re-action of the body struck must be less than the action of the striking body; otherwise, if its re-action were equal to the action, as must be the case with perfectly hard bodies, the body struck must move with the velocity of the striking body, while the latter remains at rest.—ED.

If, therefore, non-elastic soft bodies lose half their motion, or mechanical power, by change of figure in collision, and yet proceed together with half the velocity, and the non-elastic hard bodies can lose *none* in any manner whatever; then, as they must move together, their velocity must be such as to preserve the equality of the mechanic power *unimpaired,* after the stroke, the same as it was before it.

For example, let the velocity of the striking body before the stroke be 20, and its mass or quantity of matter 8; then, according to the rule deduced from the experiments in the tract on *Mechanical Power* (see Exp. 3d and 4th, p. 91), that power will be expressed by $20 \times 20 = 400$, which $\times 8 = 3200$; and if half of it is lost in the stroke, in the case of non-elastic soft bodies, it will be reduced to 1600; which $\div$ 16, the double quantity of matter, will give 100 for the square of the velocity; the square root of which, being 10, will be the velocity of the two non-elastic soft bodies after the stroke, being just one half of the original velocity, as it is constantly found to be. But, in the non-elastic hard bodies, no power being lost in the stroke, the mechanic power will remain after it, as before it, $= 3200$; this, in like manner, being divided by 16, the double quantity of matter will give 200 for the square of the velocity, the square root of which is 14·14, &c., for their velocity after the stroke, which is to 10, the velocity of the non-elastic soft bodies after the stroke, as the square root of 2 to 1, or as the diagonal of a square to its side.*

* The measure which ought to be applied in this manner is the actual quantity of motion in the moving body, which is as its mass and velocity; that is, $20 \times 8 = 160$. And, as it is supposed to strike an equal body, the result will depend on the nature of the body struck. If it be a soft body, it will experience a change of figure, till the velocity of the two bodies become equal; and then, the mass being 16, we have $\frac{160}{16} = 10$ to the common velocity of the bodies, and the rest of the power has been expended in producing the change of figure. If the bodies be soft, but partially elastic, they will not continue together after the stroke, but the sum of their velocities will be equal to half the velocity of the

It remains, therefore, now to be proved, that precisely half of the mechanic power is *lost* in the collision of non-elastic, soft bodies; for which purpose my mind suggested the following reflections. In the collision of elastic bodies, the effect, seemingly instantaneous, is yet performed in *time;* during which time the natural springs residing in elastic bodies, and which constitute them such, are bent or forced, till the motion of the striking body is divided between itself and the body at rest; and in this state the two bodies would then proceed together, as in the case of non-elastic soft bodies; but, as the springs will immediately restore themselves in an equal time, and with the same degree of *impulsive force,* wherewith they were bent in this re-action, the motion that remained in the striking body will be totally destroyed, and the total exertion of the two springs, communicated to the original resting body, will cause it to fly off with the same velocity wherewith it was struck.

Upon this idea, if we could construct a couple of bodies in such a way that they should either act as bodies perfectly elastic, or that their springs should at pleasure be hooked up, retained, or prevented from restoring themselves, when at their extreme degree of bending; and if the bodies under these circumstances observed the laws of collision of non-elastic soft bodies, then it would be proved, that one half of the mechanical power residing in the striking body would be lost in the action of collision; because the impulsive force, or power of the spring in its restitution, being cut off or suspended from acting, which is equal to the impulsive force or power to bend it (and which alone has been employed to communicate motion from one body to the other), it would make it evident, that one-half of the impulsive

striking body, their relative velocities will depend on the elasticity, and the power lost will be due to the alteration of figure. In perfectly hard bodies, no power being lost in the stroke, the body struck will proceed with the same velocity as the striking body had before the stroke, while the latter will remain at rest.—Ed.

force is lost in the action, as the other half remains *locked up* in the springs; it also follows, as a *collateral circumstance*, that be the impulsive power of the springs what it may, from first to last, yet, as one half of the *time* of the action is, by this means, cut off, in this sense also it will follow, that one half of the mechanic power is destroyed; or rather, in this case, remains locked up in the springs, capable of being *re-exerted* whenever they are set at liberty, and of producing a fresh mechanical effect, equivalent to the motion or mechanical power of the two non-elastic soft bodies after their collision.

Hence we must infer, that the quantity of mechanical power expended in displacing the parts of non-elastic soft bodies in collision, is exactly the same as that expended in bending the springs of perfectly elastic bodies; but the difference in the ultimate effect is, that in the non-elastic soft bodies, the power taken to displace the parts will be totally lost and destroyed, as it would require an equal mechanic power to be raised afresh, and exerted in a contrary direction to restore the parts back again to their former places; whereas, in the case of the elastic bodies, the operation of half the mechanic power is, as observed already, only locked up and suspended, and capable of being re-exerted without a further original accession.

These ideas arose from the result of the experiments tried upon the machine described in my said tract upon mechanic power, and were also communicated to my very worthy and ingenious friend, William Russel, Esq., F.R.S., at the same time that I shewed him those experiments in 1759: but the mode of putting this matter to a full and fair mechanical trial has since occurred; and, though some rough trials, sufficient to shew the effect, were made thereon, prior to the offering the Paper on Mechanical Power to the Society, in 1776, yet the machine itself I had not leisure to complete to my satisfaction till lately; which I mention to apologise for the

length of time that these speculations have taken in bringing forward.

DESCRIPTION OF THE MACHINE FOR COLLISION.

Fig. 1, Plate V. shews the front of the machine as it appears at rest when fitted for use.

A is the pedestal, and A B the pillar which supports the whole; C D are two compound bodies, of about a pound weight each, but as nearly equal in weight as may be. These bodies are alike in construction, which will be more particularly explained by Fig. 2. These bodies are suspended by two white fir rods, of about half an inch diameter, *e f* and *g h* being about four feet long from the point of suspension to the centre of the bodies; and their suspension is upon the cross-piece II, which is mortised through, to let the rods pass with perfect freedom; and they hang upon two small plates filed to an edge on the under side, and pass through the upper part of the rods. Their centres are at *k* and *l*, and the edges being let into a little notch, on each side the mortise, the rods are at liberty to vibrate freely upon their respective points (or rather edges) of suspension, and are determined to one plane of vibration. M N is a flat arch of white wood, which may be covered with paper, that the marks thereupon may be more conspicuous.

The cross-piece II is made to project so far before the pillar, that the bodies, in their vibrations, may pass clear of it, without danger of striking it; and also the arch M N is brought so far forward as to leave no more than a clearance sufficient for the rods to vibrate freely without touching it.

Fig. 2 shews one of the compound bodies, drawn of its full size. A B is a block of wood, and about as much in breadth as it is represented in height, through a hole in which the wood rod C C passes, and is fixed therein.

D B represents a plate of lead about three-eighths of an inch thick, one on each side, screwed on by way of giving it a competent weight. *d* B *e f g* represents the edge of a springing place of brass, rendered elastic by hard hammering; it is about five-eighths of an inch in breadth, and about one-twentieth of an inch thick. It is fixed down upon a wooden block at its end *d* B by means of a bridge plate, whose end is shewn *h i*, and is screwed down on each side of the spring-plate by screws, which, being relaxed, the spring can be taken out at pleasure, and adjusted to its proper situation. *k l* is a light thin slip of a plate, whose under edge is cut into teeth, like a fine saw or ratchet, and is attached to the spring by a pin at *k*, which passes through it, and also through a small stud riveted into the back part of the spring, and upon which pin, as a centre, it is freely movable. *m n* shews a small plate or stud, seen edgeways, raised upon the bridge-plate, through a hole in which stud the ratchet passes; and the lower part of the hole is cut to a tooth, shaped properly to catch the teeth of the ratchet, and retain it, together with the spring, at any degree to which it may be suddenly bent; and, for this intent, it is kept bearing gently downward, by means of a wire spring *o p q*, which is in reality double, the bearing part at *o* being semi-circular; from which, branching off on each side, the rod C C passes to *p*, and fixes at each end into the wood at *q*. However, to clear the ratchet, which is necessarily in the middle as well as the rod, the latter is perforated; and also the block is cut away, so far as to set the main-spring at *e* free of all obstacles that would prevent its play from the point B. The part *f g* is shewn thicker than the rest, by being covered with thin kid leather tight sewed on, to prevent a certain jarring that otherwise takes place on the meeting of the springs in collision.

Let us now return to Fig. 1, the marks upon the arch M N are put on as follow:—*o p* is an arch of a circle from the centre *l*, and *q r* an arch of a circle from the centre *k*, intersecting each other at S. Now the middle line of the marks *t*, *v*, are at the same distance from the middle line at S that the centres *k l* are: so that when each body hangs in its own free position, without bearing against the other, the rod *e f* will cover the mark at *t*, and the rod *g h* will cover the mark at *v*. From the point S, upon the arches S *p* and S *q* respectively, set off points at an equal and competent distance from S each way, which will give the middle of the mark *w* and *x*: and upon the arch S *p* find a middle point between the mark *v* and *w*, which let be *y*; and on the other side, in like manner, upon the arch S *q* find a middle point for the mark *z*; then set off the distance S *v* or S *t* from *y* each way, and from *z* each way; and from these points, drawing lines to the respective centres *l* and *k*, they will give the place and position of the marks *a*, *b*, and *c*, *d*; and thus is the machine prepared for use.

FOR TRIALS ON ELASTIC BODIES.

For this use take out the pins and ratchets from each respectively, and the springs being then at liberty, with a short bit of stick (suppose the same size as the rods) turn aside the rod *g h* with the right hand, carrying the body D upwards till the stick is upon the mark *w*, as suppose at ⊙; there hold it, and with the left set the body C perfectly at rest: in which case the rod *e f* will be over the mark *t*; then suddenly withdraw the stick, in the direction that the rod *g h* is to follow it, and the spring of the body D, impinging upon that of the body C, they will be both bent, and also restored; and the body C will fly off, and mount till its rod *e f* covers the mark *x*; the rod of the striking body D remaining at rest upon its proper mark of rest *v*, till the body C returns, when the body D will fly off in the same manner: the two bodies thus rebounding a number of times losing a part of their vibration each time; but so nearly

is the theory of elastic bodies fulfilled hereby, that the single advantage of originally pushing the rod *g h* beyond the mark *w*, by the thickness of the stick, or its own thickness, is sufficient to carry the rod of the quiescent body C completely to its mark *x*.

There are several other experiments which may be made with this apparatus, in confirmation of the doctrine of the collision of elastic bodies; which being universally agreed upon, and well known, it is needless further to dwell upon here; but respecting the application to non-elastic soft bodies, it is far more difficult to come at a fitness of materials for this kind of experiment, than it is for those supposing perfect elasticity. The conclusions, however, may be attained with equal certainty.

FOR TRIALS ON NON-ELASTIC SOFT BODIES.

For this purpose the ratchets must be applied and put in order as before described; and the springs being both put to their point of rest, let the body D be put to its mark *w* in the same manner as before described, and the body C to rest. The body D being let go, and striking the body C at rest, in consequence of the stroke, the springs being hooked up by the ratchets, they both move from their resting marks *t*, *v*, respectively towards M. Now, if they both moved together, and the rod *e f* covered the mark *c*, and the rod *g h* covered the mark *d* at their utmost limit, then they would truly obey the laws of non-elastic soft bodies; because their medium ascent would be to the mark *z*, which is just half the angle of ascent to the mark *x*; but, as in this piece of machinery, though the main or principal springs are hooked up, yet every part of them, and all the materials of which they are composed, and to which they are attached, have a degree, or more properly speaking, a certain compass of *elasticity*, which, as such, is perfect, and no motion is lost thereby.

We must not, therefore, expect the two compound bodies after the stroke to stick together without separating, as would be the case with bodies truly non-elastic and soft: but that from the elasticity they are possessed of, they will, by rebounding, be separated; but that elasticity being perfect, can occasion no loss of motion to the sum of the two bodies; so that if the body C ascends as much above its mark *c*, as the body D falls short of its mark *d*, then it will follow, that their medium ascent will still be to the mark *z*, as it ought to have been, had they been truly non-elastic soft bodies: and this, in reality, is truly the case in the experiment, as nearly as it can be discerned.

After a few vibrations, by the rubbing of the springs against one another, they are soon brought to rest; and here they would *always rest* had they been truly and properly perfect non-elastic soft bodies; but here, as in the case of these bodies, by a change of the figure and situation of the component parts, there is expended one half of the mechanic power of the first mover, yet, in this case, the other half is not *lost*, but *suspended* ready to be re-exerted whenever it is set at liberty; and that it is really and *bonâ fide one half*, and neither more or less, appears from this uncontroverted simple principle, that the power of restitution of a perfect spring is exactly equal to the power that bends it. And this may, in a certain degree, be shewn to be fact by experiment, if there were any need of such a proof; for if, when the bodies are at rest after the last experiment, the two rods are lashed together near the bottom with a bit of thread, and then the ratchets unpinned and removed; on cutting the thread with a pair of scissors they will each of them rebound, C towards M, and D towards N; and if they rebounded respectively to *z* and *y*, the mechanical power exerted would be the same as it was after the stroke, when the mean of their two ascents was up to the mark *z*; but here it is not to be expected, because not only the motion lost by the friction of the ratchets is to be deducted, because it had the effect of real non-elasticity; but also the elasticity that separated them in the stroke, which

was lost in the vibrations that succeeded: neither of which hindered the mean ascent to be to z; but yet, under all these disadvantages in the machine (if not unreasonably ill made), the rod $e f$ will ascend to d, and $g h$ to a: and hence I infer, as a positive truth, that in the collision of non-elastic soft bodies, *one half of the mechanic power residing in the striking body is lost in the stroke.**

Respecting bodies unelastic and perfectly hard, we must infer, that since we are unavoidably led to a conclusion concerning them, which contradicts what is esteemed a truth capable of the strictest demonstration; viz. that the velocity of the centre of gravity of no system of bodies can be changed by any collision betwixt one another, something must be assumed that involves a contradiction. This perfectly holds, according to all the established rules, both of perfectly elastic and perfectly non-elastic *soft* bodies; rules which must fail in the perfectly non-elastic hard bodies, if their velocity after the stroke is to the velocity of the striking body as 1 is to the square root of 2; for then the centre of gravity of the two bodies will by the stroke acquire a velocity greater than the centre

* The inference is not quite accurately drawn in this case; for the bodies separate, and move independently of one another, and the velocity of the striking body will be so divided among the effects produced, that the sum of the squares of these velocities will be equal to the square of the velocity of the striking body. Nevertheless the *momenta*, before and after the stroke, must also be equal when estimated in the same direction: and these experiments, instead of overturning the mathematical theory of collision, are most satisfactory evidences of its truth.

We have two modes of determining the conditions of moving bodies after collision. The one is, that the sum of the *momenta*, before and after collision, must be the same when estimated in the same direction. The other is, that the sum of the energies of the bodies after collision, added to the sum of the energies destroyed, must be equal to the sum of the energies of the bodies before collision. The energy of a body denoting the mass multiplied by the square of its velocity. —See Young's Natural Philosophy, Vol. II. p. 52.—Ed.

of gravity the two bodies had before the stroke in that proportion, which is proved thus.

At the outset of the striking body, the centre of gravity of the two bodies in our case will be exactly in the middle between the two; and, when they meet, it will have moved from their half distance to their point of contact; so the velocity of the centre of gravity, before the bodies meet, will be exactly one half of the velocity of the striking body: and, therefore, if the velocity of the striking body is 2, the velocity of the centre of gravity of both will be one. After the stroke, as both bodies are supposed to move in contact, the velocity of the centre of gravity will be the same as that of the bodies: and as their velocity is proved to be the square root of 2, the velocity of their centre of gravity will be increased from 1· to the square root of 2; that is, from 1· to 1·414, &c.*

The fair inference from these contradictory conclusions, therefore, is, that an unelastic hard body (perfectly so) is a repugnant idea, and contains in itself a contradiction; for to make it agree with the fair conclusions that may be drawn on each side, from clear premises, we shall be obliged to define its properties thus: That, in the stroke of unelastic hard bodies, they cannot *possibly lose any* mechanic power in the stroke, because no other impression is made than the communication of motion; and yet they *must lose a quantity* of mechanic power in the stroke; because, if they do not, their common centre of gravity, as above shewn, will acquire an *increase* of velocity by their stroke upon each other.

In a like manner, the idea of a *perpetual motion*, perhaps, at first sight may not appear to involve a contradiction in terms; but we shall be obliged to confess that it does, when, on

* This contradictory result arises out of taking an erroneous opinion respecting the collision of hard bodies, for only one of the bodies would move, as explained in note, p. 112.—Ed.

examining its requisites for execution, we find we shall want bodies having the following properties; that when they are made to *ascend* against gravitation their absolute weight shall be *less;* and when they *descend* by gravitation (through an equal space), their absolute weight shall be greater; which, according to all we know of nature, is a *repugnant* or *contradictory idea.*

EXPERIMENTAL INQUIRIES

CONCERNING THE PRINCIPLE OF

THE LATERAL COMMUNICATION OF MOTION

IN

FLUIDS,

APPLIED TO THE EXPLANATION OF

VARIOUS HYDRAULIC PHENOMENA.

By CITIZEN J. B. VENTURI,

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Translated from the French by W. Nicholson.

ADVERTISEMENT.

This interesting work (lately published at Paris, with the approbation of the French National Institute, after the examination and report of Bossut, Coulomb, and Prony,) was sent to me, some time ago, by the author. I perused it with much pleasure and instruction, and with the intention of publishing an abstract of its contents in my *Journal of Natural Philosophy, Chemistry, and the Arts;* but found it impossible to do justice to the novelty and importance of the subject, in the way of abridgement: for which reason I gave an entire translation of the whole. The Public will, no doubt, be glad to have it in a separate form. The translation is very close, and almost verbal; no liberties having in any instance been taken, except such as the different structure of the two languages, and the clear expression of the sense, demanded. The proof sheets in the Journal were examined and corrected by the work itself, and all the parts where reference is made to the engravings were, at the same time, literally compared with the plates. The same process was also repeated with this separate publication, which may,

therefore, be concluded to possess all that fidelity and accuracy which are necessary to ensure the confidence of the reader, and to render a translated work of equal utility with the original.

WILLIAM NICHOLSON.

LONDON,
April 5, 1799.

VENTURI'S

EXPERIMENTAL INQUIRIES

CONCERNING THE

MOTION OF FLUIDS.

INTRODUCTION.

The apparatus made use of in most of the following experiments is the same as that of Poleni.* It is represented at Fig. 1, Plate I. The reservoir X, of a conical form, is forty inches in diameter at $C\,E$, and thirty at $O\,P$. $F\,P$ is a broad plate of copper, the plane of which is perpendicular to the horizon; it is applied to the inside of the reservoir. The valve or flap $F\,S$, movable by the handle K, is drawn up against the side of the vessel above F, in order that it may not impede the course of the particles of the fluid, contained in the reservoir, to the aperture P. I have applied different ajutages to this aperture, according to the exigence of the case. The tubes which I applied were made of tinned iron of the best quality; the longitudinal junction of the edges was made by immediate contact, and not by overlapping, and the whole of the workmanship was executed with great care. When the aperture was simply a hole through a thin plate, the thickness of its edge did not exceed one fourth of a line.

* *De Castellis.* This treatise is reprinted in the Third Volume of Hydraulic Treatises, published at Parma. V.

The upper vessel Z serves to maintain the water of the reservoir X at the constant height of the line $C\,E$, while it flows out through P. The plug $A\,B$ is drawn more or less back, in order to regulate the introduction of the supply. The box or shelf $D\,L$ prevents this water from exciting by its fall any agitation which might influence the emission at P. The opening at Q discharges the superfluous water which might rise above the line $C\,E$. The height of the surface $C\,E$ above the centre of the orifice at P was 32·5 inches, in all cases where it is not otherwise expressed.

Most of the experiments here described were made in public at the Philosophical Theatre of Modena; various men of science were present at the rest; and the different departments of experiment were performed by several persons at the same time. One of these operators repeated the seconds audibly from the clock; another drew back the valve $S\,F$; a third regulated, by the means of the plug B, the introduction of the supply of water, so that a very thin sheet of water constantly flowed at Q. At the instant agreed upon, the passages of the water were again closed. Every experiment was repeated successively for a number of times, until the agreement of the results had removed every suspicion of error. I am assured that, even in the most complicated cases, the quantity of error could not exceed one fortieth part of the result.

The measures indicated in the course of these experiments were taken from a toise adjusted by that of the Academy, which Citizen Lalande sent me in 1783. These measures, as well as all the others of the eighteenth century, will undergo the fate which is prepared for them by the establishment of the new metre. They may be reduced to this new standard, by observing that the foot is to the metre as 100 to 308.*

* To avoid fractions, and for other obvious reasons, I have left all the numbers as they stand in the original. But, for the convenience of those who cannot

The wisest philosophers have their doubts with regard to every abstract theory concerning the motion of fluids: and even the greatest geometers avow that those methods which have afforded them such surprising advances in the mechanics of solid bodies, do not afford any conclusions with regard to hydraulics, but such as are too general and uncertain for the greater number of particular cases. Impressed with a conviction of this truth, I have attended to theory, only when it combined with the facts, and was necessary to unite them under a single point of view. Even this small portion of theory may, if the reader pleases, be rejected: and he may consider the following propositions simply as the results of experiment.

When I quote the estimable work of Citizen Bossut, on hydrodynamics, I refer to the edition of 1786.*

readily refer to the proportional magnitudes, I take the opportunity of remarking that the Paris foot royal is to the English foot as 1065·78 is to 1000; that it is divided into 12 inches, and the inch into 12 lines.—Trans.

* I consider this treatise as superior to all which before were extant. It is founded on a combination of the principles of experiment and of theory. I have profited by these principles, and several particular remarks which the same Citizen Bossut and Citizen Prony have been so good as to communicate after perusal of my memoir.—V.

EXPERIMENTAL INQUIRIES

CONCERNING

THE PRINCIPLE OF THE LATERAL COMMUNICATION OF MOTION IN FLUIDS,

APPLIED TO

THE EXPLANATION OF VARIOUS HYDRAULIC PHENOMENA.

PROPOSITION I.

The motion of a Fluid is communicated to the lateral Parts which are at rest.

NEWTON has affirmed, that when motion is propagated in a fluid, and has arrived beyond the aperture $B\,C$, Fig. 2, the motion diverges from that opening, as from a centre, and is propagated in right lines towards the lateral parts N, K, as well as towards S. The simple and immediate application of this theorem cannot be made to a jet which issues from the aperture $B\,C$ at the surface of still water. Circumstances enter into this case which transform the result of the principle into particular motions. It is nevertheless true, that the jet $B\,C$ communicates its motion to the lateral parts N, K; but it does not repel them towards P and Q, but, on the contrary, transports them along with its own stream towards S.*

* Venturi here alludes to the Cor. of the 41st proposition of Newton's Principles of Nat. Phil., but he has not attended strictly to the object of that proposition; for Newton treats only of the propagation of pressure in fluids, and not of

EXPER. 1.—The horizontal cylindric pipe *A C*, Fig. 3, is introduced into the vessel *D E F B*, which is filled with water as high as *D B*. Opposite, and at a small interval from the aperture *C*, commences a small rectangular channel of tinned iron, *S M B R*, which is open at the top *S R*; the inclined bottom *M B* rests on the edge of the vessel *B*. It is 24 lines broad; the diameter of the tube *A C* is 14·5 lines; the extremity *A* is applied to the aperture *P* of Fig. 1. The water of the reservoir being suffered to flow through the tube *A C*, the jet rises along the small channel *M B*, and flies out of the vessel in the stream *B V*. By this means a current is produced in the fluid of the vessel *D E F B*; this fluid enters into the channel *S R*, and issues by *M B V* along with the jet *A C*, so that in a few seconds the water *D B* falls to *M H*.

EXPER. 2.—Bring some very light or movable bodies near the jet of water *P Y*, Fig. 1, which issues from the aperture *P*, and falls from a certain height into the inferior vessel *R T*. It is seen that these bodies are carried along by the air which descends with the jet *P Y*. Part of this air is carried along and plunged into the water of the inferior vessel.

These experiments clearly prove that the fluid which issues by *B C*, Fig. 2, impresses its motion on the lateral parts *N*, *K*; not by impelling them towards *P*, *Q*, but by carrying them along with itself towards *S*. I call this *the lateral communication of motion in fluids*. Newton was acquainted with this communication, and has deduced from it the propagation of rotatory motion from the interior to the exterior strata of a whirlpool. Is this lateral communication of motion occasioned by the viscidity or mutual adhesion of the parts of the fluid, or their mutual engagement or intermixture, or the divergency

the actual motions of their parts. The propagation and diffusion of pressure through a mass of fluid are determined by conditions, which differ from those which determine the motions of a fluid or its parts.—ED.

of those parts which are in motion? We may perhaps say a few words on this subject when we shall have seen the effects; but in the mean time, whatever may be the cause, let us take the effect as experience points it out; let us consider it as a principle, and endeavour to apply it to some particular cases, in order to ascertain the result.

The first circumstance to which I propose to apply this principle, is the increase of expenditure or fluid issuing out of an orifice fitted with additional tubes.

PROPOSITION II.

If that part of an additional cylindric Tube which is nearest the side of the Reservoir be contracted, according to the form of the contracted Vein of Fluid, which issues through a Hole of the same Diameter in a thin Plate, the Expenditure will be the same as if the Tube were not contracted at all.

It is well known, that when the water of a reservoir is suffered to flow through a circular orifice in a thin plate, the fluid vein which forms the jet becomes contracted at a short distance from the orifice; and the diameter of the contracted vein is nearly 0·8 of the diameter of the orifice. Poleni first observed that, by applying an additional cylindric pipe to the orifice, of the same diameter as the orifice itself, and from two to four times that length, the expenditure is increased from 100 to 133. To account for this augmentation, he supposes that the fluid vein is less contracted in pipes than after passing through the thin plate. The supposition was not unreasonable; but it could not apply to the case announced in this proposition. I shall proceed to give the particulars in the following experiment.

EXPER. 3.—To the aperture P, of Fig. 1, I applied a circular orifice, 18 lines in diameter, pierced through a thin plate. Four cubical feet of water flowed into the vessel Y in 41 seconds.

I then applied to the orifice a cylindric tube of the same diameter, and fifty-four lines long. The 4 cubic feet flowed out in thirty-one seconds.

Instead of this simple cylindric tube, I applied the compound tube of Fig. 5; the parts of which have the following dimensions in lines; $A\,C = G\,I = M\,N = 18$; $D\,F = 14{\cdot}5$; $A\,B = 11$; $B\,G = 10$; $G\,M = 37$; $A\,M = 58$. With this compound tube, the expenditure of four cubic feet of water was made in 31 seconds, as with the simple cylindric tube.

The form of the conical portion $A\,C\,D\,F$ was nearly the same as that of the contraction of the vein which issues through a thin plate. The vein must, therefore, have passed through a contraction nearly equal to that of the contracted vein from a thin plate; the expenditure, nevertheless, was more abundant, in the same proportion as through the simple cylindric tube. It follows, therefore, that the velocity of the section $D\,F$, and of the whole conoid $A\,C\,D\,F$, must have been greater than that of the contracted vein from a thin plate: and it remains to be shewn what was the cause of this augmentation of velocity, which takes place within the tube, and does not manifest itself externally.

That the conical tube $A\,C\,D\,F$ does not itself cause any augmentation of expenditure, is evinced by the following:—

EXPER. 4.—The conical tube $A\,C\,D\,F$, from which the remaining part $D\,G\,M\,N\,I\,F$ was separated, was applied to the orifice P. The four cubic feet were emitted in 42 seconds, which is the time of the expense through the orifice

itself $A\,C$ in the thin plate, with the difference of one second only. This slight variation arises from its being almost impossible to make the tube $A\,D\,C\,F$ perfectly of the form of the natural contracted vein.

PROPOSITION III.

The Pressure of the Atmosphere increases the Expense of Water through a simple cylindric Tube, when compared with that which issues through a Hole in a thin Plate, whatever may be the direction of the Tube.

It has long been known, that a heavy fluid which moves in a descending cylindric pipe tends to accelerate its motion. The inferior parts tend to separate themselves from the superior, and by that means cause the pressure of the atmosphere to increase the velocity of the superior parts. This successive acceleration of gravity cannot take place in a horizontal or ascending pipe. We shall, nevertheless, find that the pressure of the atmosphere acts even in these last situations to increase the velocity of the fluid within the pipe. Certain questions of legal right, which arose in my country, respecting the quantity of water supplied by a pipe for watering lands (*canal d'arrosement*), directed my attention to this object; and, in the year 1791, I made the following experiments, publicly, in the Theatre of Natural Philosophy at Modena.

Exper. 5.—To the aperture P, Fig. 1, I applied a cylindrical pipe, fifty-four lines in length and eighteen in diameter. At the distance of nine lines from the interior orifice P, twelve small holes were made in its circumference. When these small holes were open, the four cubic feet issued out in forty-one seconds, in the same manner as through a thin plate. Not a single drop passed through any of the holes, and the stream

did not fill the tube. The holes were then closed, one after the other, with wet skin. As long as there was one hole open the expense continued the same; but when, at last, all the twelve holes were well closed, the fluid stream issued out in a body which filled the pipe, and the four cubic feet were emitted in thirty-one seconds.

Exper. 6.—To the cylindrical tube *K L V*, Fig. 6, eighteen lines in diameter and fifty-seven lines long, was joined the glass tube *Q R S T*, at the distance of eight lines from the interior orifice *K*. The glass tube was plunged in coloured water, contained in the vessel *T*. When this apparatus was applied to the aperture *P*, Fig. 1, the four cubic feet of water flowed out in thirty-one seconds. The coloured liquid *T* rose in the tube *T R* as high as S, at the height of twenty-four inches above the surface T.

The branch *R T* of the glass tube was shortened, so that *R T* was only six inches longer than *R Q*. The efflux being then permitted to take place, the coloured liquor of the vessel *T* rose through the tube *R T*, and mixed with the water which flowed from the reservoir through *K V*, both of which flowed out at *V*, and in a short time the vessel *T* was emptied.

I repeated this experiment with the compound tube, Fig. 5, and the results were the same.

Exper. 7.—The cylindrical pipe *K L V*, Fig. 6, was applied in an ascending and nearly vertical situation to the orifice *R*, Fig. 8, of the vessel *H I*, of which the end *H* communicated by an opening of considerable extent with the water of the reservoir *X*, Fig. 1. The charge on the upper extremity *V* of the tube was 27·5 inches. I inclined the tube a little from the vertical direction, in order that the jet might not fall back upon itself. The glass tube *Q R T*, Fig. 6, in this new situation was

so disposed that its lower extremity was immersed as before in the coloured liquid of the vessel T. When the efflux was permitted, the expenditure of four cubic feet was made in thirty-four seconds; and the coloured liquid rose in the tube RT to the height of near twenty inches. With the same charge of 27·5 inches the orifice of eighteen lines in a thin plate would have afforded the four cubic feet in forty-five seconds.

Exper. 8.—A cylindrical vessel of 4·5 inches diameter had in its vertical sides near the base a circular opening of 4·5 lines in diameter, opened in a thin plate of tinned iron. The surface of the water contained in this vessel was 8·3 inches above the centre of the aperture. The water was then suffered to flow out of this aperture in the thin plate, and its surface was depressed seven inches in the vessel in 27·5 seconds of time.

To the same aperture was applied a cylindric tube of the same diameter, and in length eleven lines. The vessel was filled to the same height as before, and the water being suffered to flow out, its surface was depressed seven inches in twenty-one seconds.

The same experiment was afterwards repeated in the receiver of the air-pump, under which the mercurial gauge stood at no more than ten lines in height. The surface of the water in the vessel was depressed seven inches in 27·5 seconds, whether the aperture was made in a thin plate, or whether it was provided with an additional cylindric tube.

The height of the coloured water in the tube of glass (in Experiment 6 and 7), measures the active quantity of the pressure of the atmosphere which is exerted on the surface of the water in the reservoir X to increase the expenditure. For example, in the sixth experiment we have $32·5+24$ inches charge on the orifice P; and we have nearly $\surd 32·5 : \surd 56·5 :: 31'' : 41''$

as is required by the common theory of the motion of fluids which issue out of vessels by a small aperture. The same obtains in Experiment 7.

Daniel Bernoulli made the seventh experiment in descending tubes, and in diverging conical tubes, and explained the result merely by his theory of conservation of living forces. Euler and d'Alembert observed to him, that the pressure of the atmosphere was concerned in the effect.* Though the case of the descending tube be different from that of the horizontal or ascending tube, the knowledge of the first of these two cases may nevertheless facilitate the knowledge of the second. Besides which, the causes which act in both cases are often combined together, and it is necessary to be well acquainted with both, in order to distinguish the results. On this account it is, that in the following proposition I have turned from my principal subject for a moment, to consider the first case, after which I shall return to the second.

PROPOSITION IV.

In descending cylindrical Tubes, the upper Ends of which possess the Form of the contracted Vein, the Expense is such as corresponds with the Height of the Fluid above the inferior Extremity of the Tube.

The ancients remarked, that a descending tube applied to a reservoir increases the expenditure.† Mariotte estimated that the water issues through CQ, Fig. 7, with a velocity nearly the mean proportional between the velocities arising from the two

* D'Alembert, Traité des Fluides, Art. 149.

† Calix devexus amplius rapit. Frontin. de Aquæduct. Art 36. See also the Pneumatics of Hero, in the Mathem. Vet. ed. 1693, page 157.

heights AB, AC.* Guillielmini sought for the cause of this augmentation in the weight of the atmosphere, and determined the velocity at C to be the same as would arise from the whole height AC.† In his reasoning, he supposes that the pressure at C is the same for the state of motion as for that of rest; which is not true. In the experiments he made upon this object, he paid no regard, either to the diminution of expenditure produced by the irregularity of the inner surface of the tubes, nor the augmentation occasioned by the form of the tubes themselves. By a singular accidental concurrence, one of these errors compensated for the other. I know of no other decisive experiment on this head since Guillielmini. I shall, therefore, proceed to establish the proposition upon the principle of virtual ascension combined with the pressure of the atmosphere, and that in a manner which shall be clear of every objection, of theory as well as of experiment.

Let $BLKO$, Fig. 7, represent a conical tube adapted to the form of the contracted vein;‡ the cylindrical tube $LCQK$ is of the same diameter as the contracted part. The fluid stratum LK, continuing to descend through LC, tends to accelerate its motion, according to the laws of gravitation; and, consequently, when it passes from LK to MN, it tends to detach itself from the stratum which follows, or, in other words, it tends to produce a vacuum between LK and MN; and the same effect takes place through the whole length of the tube LC. The pressure of the atmosphere becomes active, as far as is necessary to prevent the vacuum; and its action is alike, both at the surface of the fluid at A, and at the inferior extremity of the tube at C. At A it increases the expenditure, and at C it destroys

* Mouvement des Eaux, part iii. disc. 2.

† Epist. Hydrostatic. Oper. tom. i. p. 212.

‡ When I speak of the form of the contracted vein, I always mean to express the conoid formed by the fluid issuing from an orifice through a thin plate. V.

the sum of the accelerations which would be produced along *L C*, so that the fluid remains continuous in the tube.

Let *T* represent the time which the continuous column of fluid *L C Q K* employs to pass through the tube *L C*, whatever may be the velocity at *L*, and the successive acceleration from *L* to *C*. And, if we suppose this same column to return upwards from *D* to *E*, it will pass through the space *D E* = *L C* in the same time *T*, during which it will lose all the acceleration it acquired from *L* to *C*. The pressure of the column *E D*, continued for the time *T*, is, therefore, the quantity required to destroy the successive acceleration from *L* to *C*, and to prevent the fluid from ceasing to be continuous in the tube *L C*; consequently, that part of the pressure of the atmosphere, which is exerted at *C Q* to destroy the sum of the accelerations through *L C*, is equal to the pressure of a column *E D* of a fluid, homogeneous to that of the reservoir *A B*. And since the same pressure must also be exerted on the surface *A* of the reservoir, if we take *F A* = *L C*, the fluid at *L K* will possess the velocity which is proper to the height *F L* = *A C*; without considering the retardation which the internal inequalities of the tube *L C Q K* must produce.

Exper. 9.—1. The orifice *P* (Fig. 1) through a thin plate is circular, and eighteen lines in diameter. The charge of fluid above the centre of the orifice is forty inches. Four cubic feet of water were emitted in thirty-eight seconds.

2. To the orifice *P*, Fig. 1, I applied the tube *A C D*, Fig. 4, the upper end of which *A C* had the form of the contracted vein. The diameter at *A* was eighteen lines, its length, *A D*, thirty-one inches, and the situation of the tube horizontal. The expenditure of four cubical feet was made in forty-eight seconds.

3. The same orifice and the same tube were applied to the

horizontal bottom of the reservoir Fig. 7, so that the tube was vertical, and $A\ C$ = forty inches, or the height of the charge in the two former experiments. The four cubic feet flowed out in forty-eight seconds, as in the second experiment.

Exper. 10.—The last described experiment was repeated with a circular aperture of 11·2 lines in diameter. The extremity $A\ C$ of the tube Fig. 4, had the form of the contracted vein; the end A having the same diameter as that of the orifice. The other circumstances were as in the preceding cases. In the disposition, according to the first case, four cubical feet of water flowed out in ninety-eight seconds; in the second case, the time was 130 seconds; and in the third case, 129 seconds.

In each of these two experiments, the tubes and the expense of water were the same for the second and the third cases; whence it follows, that the force by which the expenditure was governed was the same in both cases. Now, the force which acts in the second case is the same as in the first; and, consequently, the same force likewise acts in the first and third cases. All the difference of the result, between the first case and the two following, arises from the retardation produced by the inequalities of the internal surface of the tubes.

Exper. 11.—The height $A\ B$, Fig. 7, being constantly 32·5 inches, and orifice $B\ O$ eighteen lines in diameter, the tube $B\ O\ C\ Q$ was applied to the orifice itself, the superior extremity of this tube having the form of the contracted vein. When the length of the tube was varied, the times of the efflux of four cubic feet of water were as in the following table.

Length of the tube *B C* in inches.	Time of efflux of four cubic feet by experiment.	Time, according to the theory, without considering the retardation.	Difference between the theory and experiment.	Retardations computed from the following experiment.
3	41″	40″	1″	1″·3
12	38	35·2	2·8	3·4
24	35	31·2	3·8	5

The fifth column of this table is calculated from the proportion of retardation produced by the irregularities of the internal surface of the tubes in the following experiment. Citizen Bossut has observed, that these retardations* increase rather in a less ratio than the velocity of the stream. This is, perhaps, the reason of the difference observed between the fourth and fifth columns.

EXPER. 12.—I applied to the orifice *P*, Fig. 1, the same tubes as in the foregoing experiment, one after another, in a horizontal position, the height of the charge being constantly 32·5 inches above the centre of the orifice. The times of emission were as in the following table.

Length of the tube *B C* in inches.	Time of efflux of four cubic feet.	Differences.
0	41″	0″
3	42·5	1·5
12	45	4
24	48	7

* Hydrodyn. Art. 622.

I must here observe, that the viscidity or mutual adhesion of the particles of the water* is of very little consequence to the increase of expenditure through the orifice *B O*, Fig. 7, by the additional tube *B C*. For, as soon as a small hole is opened at *K*, the increase of expenditure diminishes or entirely ceases, and the fluid is no longer continuous in the tube.

We will now return to tubes in the horizontal and ascending positions.

PROPOSITION V.

In an additional conical Tube, the Pressure of the Atmosphere increases the Expenditure, in the Proportion of the exterior Section of the Tube to the Section of the contracted Vein, whatever may be the Position of the Tube, provided its internal Figure be adapted throughout to the lateral Communication of Motion.

We have seen (Proposition III.) that the pressure of the atmosphere increases the expenditure through additional tubes, whatever may be their position. We shall, in the next place, examine the mode of action by which the atmosphere produces this augmentation, and determine the result from its cause. I shall begin with the case best adapted to favour the action of the atmosphere, which is that of conical diverging tubes of a certain form, which we have not yet considered.

Let the extremity *A B*, Fig. 10, of the tube *A B E F* be applied to an orifice formed in a thin plate. The part *A B C D* is nearly of the figure of the contracted vein, which form has

* Gravesande and others have attributed the increase of expenditure, through descending tubes, to the natural cohesion of the particles of water. V.

been shewn to make no perceptible alteration in the expenditure (Experiment 4). The fluid which issues through *C D* is disposed to continue its course in the cylindrical form *C D H G*. But, if the lateral parts of the diverging conical tube *C E G*, *D F H*, contain a mass of the fluid at rest, the cylindrical stream *C D H G* will communicate its motion to the lateral parts (by Prop. I.) successively from part to part. And provided the divergence of the sides *C E*, *D F*, be such as is best adapted to the speedy and complete lateral communication of motion, all the fluid contained in the truncated cone *C D E F* will at length acquire the same velocity as that of the stream which continues to issue through *C D*. On this supposition, while the fluid stratum *C D Q R*, preserving its velocity and thickness, would pass into *R Q T S*, a vacuum would be formed in the solid zone *R m r S Q o n T*. Or otherwise, if it be supposed that the stratum *C D Q R*, preserving its progressive velocity, should enlarge in *R Q T S*, this cannot happen without its becoming thinner, and detaching itself from the stratum which follows, and by that means leaving a vacuum equal in magnitude to the zone last mentioned. A similar effect would take place through the whole of the tube *C E;* and, if the quantity *C m* be supposed to be invariable, the sum of all these void spaces will be equal to the solid zone *V E x G z Y F H*.

From this consideration we see that the lateral communication of motion causes the same effect in a conical tube, whether horizontal or vertical, as gravity produces in the descending tube of Proposition IV. The atmosphere in this case, also, renders part of its pressure active on the reservoir, and at *E F*. If the action of the atmosphere upon the reservoir increases the velocity of the section *C D*, this velocity will communicate itself likewise to the whole fluid *C D F E*, and the tendency to a vacuum will take place as before; but, since the action of the atmosphere is exerted equally at *E F*, it will take away at *E F* all the velocity which is added at *C D;* so that, being deducted from the same mass, and in the same time at *E F*, the

fluid will not cease to be continuous in the pipe. It is found by computation that this will happen when the velocity of $C\,D$ is increased in the ratio of $C\,D^2$ to $E\,F^2$.

By applying the general laws of motion to the lateral fluid filaments of the stream which issues through $A\,B$, it is found that they tend to describe a curve which commences within the reservoir, for example, at A, and continues towards $C\,S\,E$. To determine the nature of this curve, it is requisite to know, and to combine together by calculation, the mutual convergency of the fluid filaments in $A\,B$, the law of the lateral communication of motion between the filaments themselves and their divergent progression from C to E. These combinations and calculations are, perhaps, beyond the utmost efforts of analysis. While the tube $A\,B\,F\,E$ possesses a different figure from this natural curve, the results of experiment will always differ more or less from the theory.

Exper. 13.—The compound tube $A\,B\,F\,E$, of the same Fig. 10, having the following dimensions in lines $A\,B = E\,F = 18$; $A\,C = 11$; $C\,D = 15{\cdot}5$; $C\,G = 49$; and this tube being applied to the orifice P, Fig. 1, under a charge of 32·5 inches, the four cubical feet of water were emitted in 27″·5.

We have seen that, in the third experiment, under like circumstances, the orifice through a thin plate afforded four cubic feet of water in 41″. The contracted vein was 0·64 of the orifice. Consequently, by following the enunciation of the theorem, the expense through the pipe $A\,B\,F\,E$ ought to be made in 26″·24. The experiment falls short in the quantity 1″·26.

Exper. 14.—Between the two conical tubes of the preceding experiment is interposed a cylindrical tube, three inches long, and 15·5 lines in diameter. The interposition of the cylinder

between the two cones was made as in Fig. 13. This addition retarded the expenditure 1″, the time now being 28″·5.

Exper. 15.—The charge of the reservoir being constantly 32·5 inches, the portion of the tube *A B C D*, Fig. 11, had the same dimensions as before; the tube *C D F E* was seventy-eight lines in length, and its diameter *E F* twenty-three lines. To this horizontal tube I added three glass tubes; the first *D X* at *C D;* the second *N Y* at the distance of twenty-six lines from the first; and the third *O Z* at twenty-six lines distance from the second. The lower extremities of these three tubes were plunged in the mercury of the vessel *Q*. When the water was suffered to flow through the tube *A E F B*, the mercury rose fifty-three lines in the tube *D X*, 20·5 in *N Y*, and seven in *O Z*. These quantities correspond with sixty-two inches height of water in *D X*, twenty-four inches in *N Y*, and 8·1 in *O Z*. The expenditure of four cubic feet was effected in 25″.

I cut off the portion *P N F E* of the tube, and the remaining pipe *A B N P* emitted the same quantity in 31″.

In the truncated conical tube *A C P B D N*, the section *P N* is to the section of the contracted vein (namely, 0·64 of the section *A B*) as 41″ to 30″. In the experiment with this last truncated tube the retardation is consequently no more than 1″ less than the theory.

In the entire tube *C D F E* we have $\sqrt{62 + 32{\cdot}5} : \sqrt{32{\cdot}5} = 41'' : 24''$. The difference of thirty-eight inches' elevation of water in the two tubes *D X*, *N Y*, must arise from the motion of the fluid from *C* to *P;* it is 1-13th less than by the theory. The loss is successively greater in the two portions *P Q*, *Q E*. The reason of this is, that the stream descends as it moves from *C D*, so that the lateral communication not being made uni-

formly through the whole of any one section, the different parts of the current acquire irregular motions, and even eddies within the tube; whence the jet comes forth by leaps, and with irregular scattering.—These uncertain motions cannot be reduced to the theory, and manifest themselves the more, the longer or the more diverging the sides of the tube. The effects, consequently, remain to be ascertained by experiment.

EXPER. 16.—I constructed a tube *C D F E* as before (Fig. 11), 148 lines long, and twenty-seven lines in diameter at *E F*, the rest of the apparatus being the same as in the foregoing experiment. The expenditure of four cubical feet was effected in 21″: the inequality and irregularity of motion in the stream were greater in this experiment than in the foregoing.

It was useless to prolong the tube *C D F E* beyond 148 lines; for the stream did not, in that case, fill the portion of tube added beyond that length, and the expenditure remained constantly at 21″. This expenditure is nearly double what took place through the simple aperture in a thin plate; and it is the greatest I have been able to obtain by additional tubes, the axis of which had a horizontal position under a charge of 32·5 inches.

It is true, that by prolonging the tube *C D F E* to the length of 204 lines in the horizontal position, the four cubic feet flowed out in 19″. But, to obtain this effect, I found it necessary to fix a prominence within the tube at *O*, which forced the fluid to fly upwards, and by that means to fill the whole tube.

EXPER. 17.—In this experiment the horizontal tube *C D F E*, Fig. 11, was more divergent than in the foregoing trials. It was 117 lines long, and thirty-six lines in diameter at *E F*. The rest of the apparatus was the same as before. The expenditure was made in 28″; the stream did not fill the whole

section $E F$. The result was the same when successive portions of the pipe were cut off, until $C E$ was no longer than twenty lines, and the external diameter eighteen lines. In this case the stream filled the pipe, and the expenditure was also made in 28′.

When the length $C E$ was twenty lines, its external diameter $E F$ was increased to twenty lines. In this case the stream was detached from the sides of the tube, and the expense of four feet took place in forty-two seconds, as in the sixth experiment.

These experiments teach us, that by varying the divergence of the sides of tubes, the lateral communication of motion has a minimum and a maximum of effect. The minimum is seen in the last experiment. It appears that the lateral communication ceases to produce its effect, when the angle made by the sides of tube with each other, exceeds sixteen degrees. The thirteenth experiment nearly determines the maximum of the effect, when the same angle is about three degrees. These limits may also, perhaps, in a small degree depend upon some function of the velocity.

PROPOSITION VI.

In cylindrical Pipes the Expenditure is less than through conical Pipes, which diverge from the place of the contracted Vein, and have the same exterior Diameter.

The general theory is the same for both these forms of tubes; but the loss of living force is greater in the cylinder, and the effect of the communication of motion in these tubes cannot approach its maximum as in the cone. Let the tube $A C M N$,

Fig. 5, Plate I., have the form of the contracted vein in *A C F D;* the cylindrical part *G I N M* has its diameter *M N*, greater than *D F*. By the reasoning made use of in the preceding proposition, it is proved that the lateral communication of motion tends to produce a vacuum in the solid zone *R O Y S X Q T Z*. If the communication of motion in this tube were completely made, it would follow that the pressure of the atmosphere would increase the velocity of the contracted vein in the ratio by $D F^2$ to $M N^2$.

But the form itself of the cylindrical pipe always destroys a notable part of the effect; for the fluid filaments *A D*, in turning through the curve *D R*, proceed briskly to strike the sides of the tube *G M* at *R*, where they lose part of their motion. In the space *D G R*, eddies, or circular whirls, are produced, as in a basin which receives water by a channel. These eddies produce, to a certain extent, a failure of the effect, and retard the efflux of the stream. A much less increase of the expenditure takes place in the cylindrical tube than would answer to the ratio of $D F^2$ to $M N^2$.

Exper. 18.—A notion may be formed of these internal shocks and eddies in the cylindric tube, and their effects on the efflux of the fluid, if attention be paid to the following table of the expenditure through the different additional tubes in the horizontal position. All these tubes have the diameter of their two extremities = 18 lines; they were all provided with the conical tube of the form of the contracted vein at their inner extremity, excepting that of Fig. 6. The charge was always 32·5 inches above the centre of the orifice.

Table of the Times employed in discharging four cubic Feet of Water through the different Ajutages.

Through the orifice in a thin plate.....	41″
Through the simple tube of Fig. 6	31″

Through the tube of the form of Fig. 5	31″
After having amended (adouci) the conical divergent part, *D F I G*, of the same tube.....	30″
Through the tube Fig. 9	32″·5
Through the conical tube of the form Fig. 10	27″·5
Through the tube Fig. 5, the portion *G I N M* being 23·5 lines in diameter, and eighty-four in length, the rest as before	27″

It may, perhaps, be demanded, whether, in the internal part of the simple cylindrical tube *K L V* of Fig. 6, there be the same augmentation of velocity, and the same contraction of the stream as in the compound tube of Fig. 5? By reasoning according to the principles we have established, I think, 1. That in the section *K L* of Fig. 6, there is the same increase of velocity as we have seen (Prop. II.) take place in the section *A C* of Fig. 5. The direction of the fluid particles which pass through these two sections must be the same in both cases, because this direction can depend only on the impulse received within the reservoir, which is the same in both. 2. In Fig. 6, the fluid particles, after having passed through the section *K L*, begin immediately to experience the effect of the lateral communication of motion. They must, therefore, deviate laterally through the curve *L x z*, before they arrive at the place of contraction which they assume at *D F*, Fig. 5, and which they likewise assume when the orifice is made in a thin plate. If we imagine a tube of glass *y K*, one extremity of which is applied at K, Fig. 6, and the other extremity open in the interior part of the reservoir, it will be seen that the pressure of the atmosphere, which is exerted upon the coloured fluid *T*, must likewise act on the surface of the reservoir, and join the pressure of the fluid in the reservoir to press the water into the tube *y K*, as it presses the coloured liquor into *T S*. The pressure of the atmosphere must, in the same manner, augment the impulse of all the fluid particles which arrive at *K L*, and, consequently, must increase the expenditure.

Since the cheeks and eddies in an additional cylindric tube must always destroy a part of the active force of the fluid, it follows, that the fluid column issuing out of the tube can never acquire the whole velocity which is due to the actual charge, and is observed nearly entire in the orifices through a thin plate; and the diminution of velocity corresponds with the increase of the time beyond that indicated by the theory, as may be seen in the following:

EXPER. 19.—The orifice P, Fig. 1, being made through a thin plate, and the vertical height PM, being fifty-four inches, the distance MN of the jet was 81·5 inches. Having applied to the same orifice the cylindrical tube of Fig. 5, and the perpendicular PM being let fall from the external orifice of the tube, the distance MN was found to be sixty-nine inches. According to the theory, the expenditure of four cubical feet through this tube ought to have taken place in 26″·24, but it really employed 31″. And the proportions 31″ : 26″·24 = 81·5 : 69 nearly.

The same observation may be made on an experiment of Michelotti (tom. ii. pages 22 and 23), PM being 19·33 feet, and the water issuing through an orifice in a thin plate MN was 23·2 feet; it was no more than twenty when an additional cylindric tube was applied which had not even the proper length.

It is evident, that the theory of the lateral communication of motion must likewise apply in the same manner to descending and ascending tubes, whenever their form admits of this lateral communication. In descending tubes, we must add the increase of expenditure occasioned by this cause to that which is produced by the acceleration of gravity, and which we have estimated in Proposition IV. In ascending tubes, gravity acts in a contrary direction, and, consequently, its effect must be deducted from that of the lateral communication. Experiment 7

relates to ascending tubes. The following relate to other positions.

EXPER. 20.—The tube *A B F E* of Fig. 11, Experiment 15, was applied in the place of the tube *B C Q O*, in Fig. 7. The height of the water in the reservoir above the lower extremity of the tube was 41·5 inches. The four cubical feet of water were emitted in 22″.

I applied the same conical tube *ABFE*, Fig. 11, to the orifice *R*, Fig. 8, to form an ascending jet a little inclined from the perpendicular. The height of the water of the reservoir above the upper extremity of the tube was twenty-three inches. The expenditure of four cubical feet was made in 30″.

The time of the expenditure in Experiment 15, was 25″. And by comparing it with the present, we find nearly $\sqrt{41{\cdot}5} : \sqrt{32{\cdot}5} = 25'' : 22''$. And $\sqrt{23} : \sqrt{32{\cdot}5} = 25'' : 30''$.

EXPER. 21.—The orifice *R*, Fig. 8, was circular, and 4·5 lines in diameter; the charge was 31·7 inches, and the jet declined a little from the perpendicular. The orifice being through a thin plate, afforded a cubical foot of water in 161″. With an additional cylindrical tube of the same diameter, and ten lines in length, the cubical foot of water was emitted in 121″.

Under a charge of fifty-six inches, the same orifice afforded by the vertical jet, a cubical foot in 123″ through the thin plate, and in 91″ with the same additional tube.

These two results being combined, give for the expenditure of vertical jets a mean ratio, between the thin plate and the cylindrical ajutage, of 100 to 134, which is also the ratio between the horizontal jets.

EXPER. 22.—I applied the glass tube $Q\,R\,T$, Fig. 6, to the point S, Fig. 5, of the compound tube $A\,C\,M\,N$, the distance $B\,S$ being twenty-four lines. In this situation the fluid T no longer rises in the tube. This proves that the lateral translation of the fluid in the cylindrical tube is made very near the place where the vein is contracted, and that, consequently, $D\,R$ must briskly strike the side $G\,M$.

By this experiment we see that the distance $B\,R$ at which the oblique filaments strike the sides of the tube, does not amount to twenty-four lines. Supposing $D\,O = 20$ lines, the time which the particle D employs to pass through the space $D\,O$ in my experiments is less than $0''{\cdot}01$. Let us decompose the curve-lined motion $D\,R$ according to the lines $D\,O$, $O\,R$. Let us suppose the acceleration through $O\,R$ to be uniform, and it will be found that this acceleration is at least five times as great as that of heavy bodies. If the lateral force through $O\,R$ were simply the mutual attraction of the particles of the water, this attraction in the particle D must not only overcome the inertia of the particle itself, but likewise that of the other particles nearer the axis, which follow D in its deviation through $D\,R$, and impress upon them a much greater sum of acceleration than that of gravity. Now the force of attraction of one particle of water is not greater than the natural gravity of a thread of water of the length of one line at most. The lateral communication of motion, which is the cause of the acceleration through $O\,R$, is, therefore, much greater than could have been produced by the mutual attraction of the particles of water.

PROPOSITION VII.

By means of proper Ajutages applied to a given cylindric Tube, it is possible to increase the Expenditure of Water through that Tube in the proportion of Twenty-four to Ten, the Charge or Height of the Reservoir remaining the same.

I shall here give an account of the different precautions necessary to be taken when the expenditure of water through a cylindrical tube of a given length is required to be the greatest possible.

1. The inner extremity of the tube $A\,D$ (Fig. 13), must be fitted at $A\,B$ with a conical piece of the form of the contracted vein;* this increases the expenditure as 12·1 to 10. Every other form will afford less. If the diameter at A be too great, the contraction will be made beyond B, and the section of the vein will be smaller than the section of the tube.

2. At the other extremity of the pipe $B\,C$ apply a truncated conical tube $C\,D$, of which let the length be nearly nine times the diameter C, and its external diameter D must be 1·8 C. This additional piece will increase the expenditure as 24 to 12·1, (Experiment 16). By this means the quantity of water will be increased by the two ajutages $A\,B$, $C\,D$, in the proportion of twenty-four to ten.†

* Bossut, Art. 509.

† The idea that this ratio will apply to long pipes is erroneous; for it must obviously decrease as the force necessary to produce motion increases; and, therefore, in long pipes will have very little effect. It may also be inferred that the effect of a truncated tube applied at the outer extremity will depend considerably on the velocity of the efflux. The Roman law, mentioned in the next

At Rome, the inhabitants purchase the right of conveying water from the public reservoirs into their houses. The law prohibits them from making the pipe of conveyance larger than the aperture granted them at the reservoir, as far as the distance of fifty feet.* The legislature was, therefore, aware, that an additional pipe of greater diameter than the orifice would increase the expenditure; but it was not perceived that the law might be equally evaded by applying the conical frustum $C\,D$ beyond the fifty feet.

From the second rule we learn, that it is not proper to make the flues of chimneys too large in the apartments; but that it will be sufficient if they be enlarged at their upper terminations, according to the form $C\,D$, Fig. 13. This divergency of the upper part will carry off the smoke very well, even when it is not practicable to afford chimneys of sufficient length to the upper apartments. The same observation is applicable to chemical furnaces for strong fire.

3. The pipe $B\,C$ ought to be straight, without elbows or curvatures. To the experiments which Bossut has made on this head,† I shall add the following:—

Exper. 23.—The two tubes $A\,B\,C$, $D\,E\,F$, Fig. 14. Plate II, are 15 inches long; their diameter is 14·5 lines. The conical portions A, D, have the form of the contraction of the vein of fluid, and are applied to the orifice P, Fig. 1, which is 18 lines in diameter, with 32·5 inches depth, or charge of superincumbent fluid. The elbows, or flexures, $B\,C$, $E\,F$, are made

paragraph has most likely limited the distance to that where the increase of expenditure would be inconsiderable for the size of pipe commonly used at the time.—Ed.

* Fontin. de Aquæduct. Art. 205, 106 et 112.

† Art. 631, et seq.

in the plane of the horizon. These two pipes are made of copper soldered with silver, and the workmanship carefully executed. The curvature $B\,C$ was drawn out or bended, into the form of a quarter of a circle, by filling the tube with melted lead in order that it might preserve its diameter during the act of bending. The elbow $D\,E\,F$ is constructed in a right angle. The expenditure through these two tubes was compared with that afforded through a right-lined cylindrical tube of similar dimensions, and in like circumstances. The four cubical feet of water flowed out of the cylindrical tube in 45″; out of the curved tube $A\,BC$ in 50″; and out of the angular tube $D\,E\,F$ in 70″.

It is of importance that the tube $B\,C$, Fig. 13, Plate I, should be of an equal diameter throughout. It is not enough that care be taken that there should be no contraction; it is also necessary that it should not be enlarged at any part. For such enlargements have nearly the same bad effect in the expenditure as contractions. The pipe $A\,O$, Fig. 12, affords a much less quantity of fluid with the dilatations $D\,E$, $H\,I$, than if it were of a diameter equal to that at B throughout its whole length. The following experiment agrees with the theory.

Exper. 24.—The circular orifice A, Fig. 12, has the form of the contraction of the vein, and the remaining part of the tube is interrupted by various enlargements of its diameter. This tube is applied to the aperture P, Fig. 1. The dimensions of its parts measured in lines are as follow:—Diameter at $A = 11{\cdot}2$. Diameter at B, C, F, G, &c., $= 9$. Length of $B\,C = F\,G$, &c., $= 20$. Length of $C\,D = E\,F = G\,H$, &c., $= 13$. Diameter of the enlarged parts $= 24$. The length of each of the enlarged parts was variable. The first time of trial it was 38 lines, the second 76, and the result of the experiment was the same in both cases.

No. of enlarged parts.	Time during which four cubical feet issued out.
0	109′
1	147″
3	192″
5	240″

I afterwards applied to the same orifice a tube, having the same form, and the same diameter, as $A\,B\,C$, but cylindrical throughout, without any enlargements, and its length was thirty-six inches, the same as that of the tube with five enlarged parts; and, in this case, the expenditure of four cubical feet was made in 148″.

When the fluid passes from C to the middle of the enlarged part $D\,E$, part of the motion is diverted from the direction $C\,F$ towards the lateral parts of the enlargement. This part of the motion is consumed in eddies, or against the sides. Consequently, there remains so much the less motion in the following branch $F\,G$. This is also the cause which destroys or weakens the pulse in the arteries beyond an aneurism.

From this consideration we are justified in concluding, that if the internal roughness of a pipe diminishes the expenditure, the friction of the water against these asperities does not form any considerable part of the cause. A right-lined tube may have its internal surface highly polished throughout its whole length; it may every where possess a diameter greater than the orifice to which it is applied; but, nevertheless, the expenditure will be greatly retarded, if the pipe should have enlarged parts or swellings. This is a very interesting circumstance to which, perhaps, sufficient attention has not been paid in the construction of hydraulic machines. It is not enough that elbows and contractions are avoided; for it may happen, by an intermediate enlargement, that the whole advantage may be lost, which may have been procured by the ingenious dispositions of the other parts of the machine.

PROPOSITION VIII.

In the Machine for blowing by means of a Fall of Water, the Air is afforded to the Furnace by the accelerating Force of Gravity, and the lateral Communication of Motion, combined together.

The academy of Toulouse, in the year 1791, invited philosophers to determine the cause and the nature of the stream of air which is produced by the fall of water in certain forges. I propose, in this place to develope the complete action of this kind of blowing apparatus, and to ascertain the best form of construction. Kircher is the first I know of, who has explained the production of wind by a fall of water.* Barthes, the father, has given a theory which appears to me to be defective in many respects.† Dietrich was of opinion, that this wind is produced by the decomposition of water.‡ Fabri had a similar notion in the last century.§ Most philosophers are well acquainted with this kind of engine.‖

I shall begin with an idea, the foundation of which did not escape the penetration of Leonardo da Vinci. Suppose a number of equal balls to move in contact with each other along the horizontal line *A B*, Fig. 15, Plate II. Imagine them to pass

* Mundus Subterr. lib. xiv. cap. 5, edit. 1662.

† Mémoires des Savans étrangers, vol. iii. p. 378.

‡ Gites de Minerai des Pyrénées, pp. 48, 49.

§ Physic. Tract. i. lib. ii. prop. 243.

‖ Art. des Forges, part. ii. — Mariotte des Eaux, part i. disc. iii. — Transact. No. 473, &c.

with an uniform motion, at the rate of four balls in a second. Let us take BF, equal to 16 feet English. During each second, four balls will fall from B to F, and their respective distances in falling will be nearly $BC = 1$, $CD = 3$, $DE = 5$, $EF = 7$. We have here a very evident representation of the separation and successive elongation, which the accelerating force of gravity produces between bodies which fall after each other.

The rain-water flows out of gutters by a continued current; but during its fall it separates into portions in the vertical direction, and strikes the pavement with distinct blows. The water likewise divides, and is scattered in the horizontal direction. The stream which issues out of the gutter may be one inch in diameter, and strike the pavement over the space of one foot. The air which exists between the vertical and horizontal separations of the water which falls is impelled and carried downwards. Other air succeeds laterally, and, in this manner, a current of air or wind is produced round the place struck by the water. I went to the foot of the cascades which fall from the Glacière of La Roche-Mêlon, on the naked rock at La Novalêse, towards Mount Cenis, and found the force of the wind to be such as could scarcely be withstood. If the cascade fall into a basin, the air is carried to the bottom, whence it rises with violence, and disperses the water all round in the form of a mist.

The water which is precipitated in the hollow internal parts of mountains carries the air with it, which, afterwards issuing forth from apertures at the foot of the mountain, produces those natural blasts, those *ventaroli**, which are most frequently

* These *ventaroli* are sometimes produced by the difference of temperature between the air of the cavern and the external air.—V. From the effects, they seem to be oftener produced by this last cause than by a fall of water. On this subject in general, namely, the cold winds which issue out of the earth, see Nicholson's Philos. Journal, i. 229.—Trans.

observed in the volcanic mountains, because these mountains are most commonly hollow within.

Let *B C D E*, Fig. 16, represent a pipe, through which the water of a canal *A B* falls into the lower receiver *M N*. The sides of the tube have openings all round, through which the air freely enters to supply what the water carries down in its fall. This mixture of water and air proceeds to strike a mass of stone *Q*: whence rebounding through the whole width of the receiver *M N*, the water separates from the air, and falls to the bottom at *X Z*, whence it is discharged into the lower channel or drain, by one or more openings, *T*, *V*. The air, being less heavy than the water, occupies the upper part of the receiver, whence being urged through the upper pipe *O*, it is conveyed to the forge.

Exper. 25.—I formed one of these artificial blowing engines of a small size. The pipe *B D* was two inches in diameter, and four feet in height. When the water accurately filled the section *B C*, and all the lateral openings of the pipe *D B E C* were closed, the pipe *O* no longer afforded any wind.

It is, therefore, evident that in the open pipes the whole of the wind comes from the atmosphere, and no portion is afforded by the decomposition of water. Water cannot be decomposed and transformed into gas, by the simple agitation and mechanical percussion of its parts. The opinions of Fabri and Dietrich have no foundation in nature, and are contrary to experiment.

It remains, therefore, to determine the circumstances proper to drive into the receiver *M N*, the greatest quantity of air, and to measure that quantity. The circumstances which favour the most abundant production of wind are the following:

1. It is known that in the parabola, if the increment of x be assumed as constant, that of y will decrease nearly in the ratio of $\frac{1}{\sqrt{x}}$. The separation of the balls, in Fig. 15, is more rapid in the upper spaces of the fall than in the lower. In order, therefore, to obtain the greatest effect from the acceleration of gravity, it is necessary that the water should begin to fall at $B\,C$, Fig. 16, with the least possible velocity; and that the height of the water $F\,B$ should be no more than is necessary to fill the section $B\,C$. I suppose the vertical velocity of this section to be produced by a height or head equal to $B\,C$.

2. We do not know, by direct experiment, the distance to which the lateral communication of motion between water and air can extend itself; but we may admit with confidence, that it can take place in a section double that of the original section, with which the water enters the pipe. Let us suppose the section of the pipe $B\,D\,E\,C$ to be double the section of the water at $B\,C$; and in order that the stream of fluid may extend and divide itself through the whole double section of the pipe, some bars, or a grate, are placed in $B\,C$, to distribute and scatter the water through the whole internal cavity of the pipe.

3. Since the air is required to move in the pipe O with a certain velocity, it must be compressed in the receiver. This compression will be proportioned to the sum of the accelerations, which shall have been destroyed in the interior part $K\,D$ of the pipe. Taking $K\,D = 1{\cdot}5$ feet, we shall have a pressure sufficient to give the requisite velocity in the pipe O. The sides of the portion $K\,D$, as well as those of the receiver $M\,N$, must be exactly closed in every part.

4. The lateral openings in the remaining part of the pipe $B\,K$, may be so disposed and multiplied, particularly at the upper part, that the air may have free access within the tube.

I will suppose them to be such that 0·1 foot height of water might be sufficient to give the necessary velocity to the air at its introduction through the apertures.

All these conditions being attended to, and supposing the pipe BD to be cylindrical, it is required to determine the quantity of air which passes in a given time through the circular section KL. Let us take in feet $KD = 1{\cdot}5$; $BC = BF = a$; $BD = b$. By the common theory of falling bodies, the velocity in KL will be $7{\cdot}76\ \sqrt{(a+b-1{\cdot}4)}$; the circular section $KL = 0{\cdot}785a^2$. Admitting the air in KL to have acquired the same velocity as the water, the quantity of the mixture of the water and air which passes in a second through KL is $= 6{\cdot}1a^2\ \sqrt{(a+b-1{\cdot}4)}$. We must deduct from the quantity $(a+b-1{\cdot}4)$ that height which answers to the velocity the water must lose by that portion of velocity which it communicates to the new air laterally and constantly introduced ; but this quantity is so small that it may be neglected in the calculation. The water which passes in the same time of one second through BC is $= 0{\cdot}4a^2\ \sqrt{(a+0{\cdot}1)}$. Consequently, the quantity of air which passes in one second through KL, will be $= 6{\cdot}1a^2\ \sqrt{(a+b-1{\cdot}4)} - 0{\cdot}4a^2\ \sqrt{(a+0{\cdot}1)}$. taking the air itself, even in its ordinary state of compression, under the weight of the atmosphere. It will be proper, in practical applications, to deduct one-fourth from this quantity ; 1, on account of the shocks which the scattered water sustains against the interior part of the tube, which deprive it of part of its motion ; and 2, because it must happen that the air in LK will not, in all its parts, have acquired the same velocity as the water.

If the pipe O do not discharge the whole quantity of air afforded by the fall, the water will descend at XZ; the point K will rise in the pipe, the afflux of air will diminish, and part of the wind will issue out of the lower lateral apertures of the pipe BK.

I shall not here examine the greater or less degree of perfection of the different forms of water-blowing machines which are used at various iron forges, such as those of the Catalans, and elsewhere. These points may be easily determined from the principles here laid down.

PROPOSITION IX.

It is possible, by means of a Fall of Water, to drain a Piece of Ground, without the help of Machines; even though the Ground should lie on a lower Level than the established Current below the Fall.

The means of doing this are pointed out in the first experiment of this treatise. We have seen that the water contained in the vessel $DEFB$, Fig. 3, Plate I., issues through the channel MBV, which is higher than the surface of the water itself, because the fluid which passes through AC carries with it the water contained in the vessel.

In the artificial fall, which is procured in channels to give motion to mills, when the water rushes down by a rectangular trunk of wood, $DBCF$, Fig. 17, placed nearly horizontal in the middle of the lower channel, the surface of the water at K is one or two feet beneath the inferior current (or back-water) FL.* The water at F tends to return and descend along FK; but the current, by its lateral action, constantly carries it away, and does not permit it to slide down to K. If an opening G be made in the lateral sides of the trunk, the waters

* This depression of the level has already been noticed in K. Guilielmini, *Della Natura dè Fiumi*, cap. vii. fig. 46. Bossut, art. 721. The wheel alluded to in the text must I presume be of that kind which we call a breast-wheel.—(Trans.)

from lands lower than the current of the inferior stream $F L$ may be drained off. In a commission with several of my colleagues, I once proposed, that this principal should be applied to a case in practice. The project was adopted, and the drainage succeeded very well.

The rectangular conduit $D B F C$ must be prolonged to a certain extent along the lower channel, otherwise the water might flow back from F to K, and oppose the drainage through G. The millwrights are aware of the utility of this prolongation. Experience has taught them, that it prevents the water from returning back so readily in the time of floods, which might check the motion of the water-wheel. For this purpose, they make the upper part $D F$ at the height of the waters which the mill-stream can resist or support. The town of Final, in the territory of Modena, having charged me with the direction of changing the course of part of the waters of the Panaro, which the circumstances of the town required to be done; I availed myself of this prolongation of the tail-pipe $D F$, together with other contrivances, to maintain the action of mills in the new channel; and I succeeded, not only beyond the expectation of the inhabitants, but even beyond my own hopes.*

* Particular modes of applying this principle to keep off the back-water in floods, have been claimed by different millwrights. Mr. Burns, of Renfrewshire, is mentioned by Dr. Brewster as the first, and Mr. Perkins, as a second proposer of such a method; the latter was rewarded by the Society of Arts, and his method is described in their Transactions, vol. xxxviii.—Ed.

PROPOSITION X.

The Eddies of the Water in Rivers are produced by Motion communicated from the more rapid Parts of the Stream to the lateral Parts, which are less rapidly moved.

Few authors have examined the cause and the effects of the eddies of water in rivers; and those who have undertaken this investigation, do not appear to have been very happy in their researches.

The water which moves in the channel MNH, Fig. 19. meets the obstacle BA, which impedes its course, and causes it to rise and discharge itself in the direction AC with an increased velocity. Suppose the water in $BDCA$ to be dormant, the current AC communicates its motion to the lateral particles E (Prop. I.) and conveys them forward; the surface of the dormant water becomes depressed at E, and the most remote particles towards D are urged, according to the laws of the equilibrium of fluids, to fill the depression. The current AC continues to carry them off, and the space $BDCA$ continues to be exhausted. The water of the current AC, by virtue of the same laws, is acted upon by a constant force, which urges it towards the cavity E, while its natural course or projection carries it towards AC. Under the agency of these two forces, the water AC acquires a curve-lined motion in CD, and descends, as it were, through an inclined plane, becoming retrograde in DE, whence it would proceed to strike the obstacle BA, and the current AC, and afterwards it would undergo several oscillations previous to acquiring a state of equilibrium and repose. But the current AC continues its lateral action: a second time it draws away the water through CD into E, and forces it to renew its motion through the curve CDE; in which manner the eddy continues without ceasing.

If the river should pass through a contraction of its bed at *N*, eddies will be produced on both sides at *P* and *Q*, similar to those we have contemplated at *D C*.

Suppose the stream of water, after having struck the bank *G H*, to be reflected into a new direction *H S*, the lateral communication of motion will excite eddies in the angle of reflection *R*.

When two currents of unequal velocity meet obliquely in the middle of the river, the most rapid current will produce eddies in that which is the least rapid.

Suppose a stream of water to flow over a bed of unequal depth. If the longitudinal section of the inequalities of the bottom exhibit a gentle slope, as at *A B C*, Fig. 20, the superior water will impress its motion by lateral communication upon the inferior water, which is near the bottom, beneath the line *A C*, and a current will take place through the whole depth of the section *M B*. The current which is formed near the bottom at *B*, is turned out of its course by the slope *B C*, and proceeds to rise above the surface at *Q ;* sometimes in the form of a curling wave or vertical whirlpool. If the extremities of the hollow place form an abrupt angle, as *D E, F G*, eddies will be produced even at the bottom, in the vertical direction at *D*, and sometimes also at G. These phenomena may be observed in an artificial channel with glass sides.

Every eddy destroys a part of the moving force of the current of the river. For the water which descends by a retrograde motion, in the inclined plane *CDE*, Fig. 19, cannot be restored in the direction of the current of the river, but by a new impulse. It is, as it were, a ball which is forced to rise on an inclined plane, whence it continually falls back again to receive new impulsions. It is the labour of Sisyphus.

Hence, I deduce, as a primary consequence, *that in a river, of which the course is permanent, and the sections of its bed unequal, the water continues more elevated than it would have done if the whole river had been equally contracted to the dimensions of its smallest section.* The cause of this phenomenon is the same as that which retards the expenditure through the tube with enlarged parts. (Prop. VII. No. 4.) The water which descends from the elevation above the contracted part N, into the basin $P\,Q$, Fig. 19, loses nearly the whole of the velocity it acquired by descending from it; because the narrow part has a curved slope towards the lower part of the river, which directs the velocity of the stream in a horizontal direction. Guilielmini has well remarked, that a fall does not influence the velocity of the lower stream, because the eddies of the water in the basin $P\,Q$ destroy the velocity produced by the fall. This velocity increases the depth, and enlarges the width of the channel at $P\,Q$. Eddies are formed on each side, at the bottom, and at the surface; both in the horizontal and vertical directions. It would be to no purpose to attempt to prevent this hollowing out and enlargement of the channel by such a fall, by adopting the means of close walls; for the basin would then obtain its enlargement, where these constructions might end.

If the channel have a number of successive contractions and dilatations, $M\,N$, without cascade or dam, there will be still formed, at each dilatation, eddies which will diminish the velocity more than if the channel had an uniform section equal to that in M or N. It will, therefore, follow, that the surface of the water after each dilatation, must rise in order to recover the velocity it lost by the eddies. If we call the height to which the water must rise above the elevation necessary to have overcome the retardations of a bed of uniform section, $= a$, and that the number of equal and successive alternate dilatations and contractions be $= m$, the height of the rise in the stream thus alternately dilated beyond that of the same river

uniformly contracted will be = *am*. I here suppose the bottom of the river to be uniform. If this bottom be of such a nature as to be attacked by the current, the contracted parts will be hollowed out, and the matter will be deposited in the enlarged parts.

The second consequence which I draw from the principle here established, respecting the loss of force caused by the eddies is of considerable importance in the theory of rivers, and appears to have been neglected by those who have treated on this subject. The friction of water along the wet banks, and over the bottoms of rivers, is very far from being the only cause of the retardation of their course, which, consequently, requires a continued descent to maintain its velocity. One of the principal and most frequent causes of retardation in a river, is also produced by the eddies which are incessantly formed in the dilatations of the bed, the cavities of the bottom, the inequalities of the banks, the flexures or windings of its course, the currents which cross each other, and the streams which strike each other with different velocities. A considerable part of the force of the current is thus employed to restore an equilibrium of motion which that current itself does continually derange.

PROPOSITION XI.

If the Water of a Reservoir, which flows through a horizontal Aperture, be influenced by any foreign Motion, it will form a hollow Whirl above the Orifice itself.

Citizen Bossut has given a good description of this kind of eddy.* It is of a different nature from those considered in the foregoing proposition; but the causes are, in some respects,

* Hdyroydn. No. 432.

similar, for which reason I propose to attend to them more particularly in this place.

Let DQ, Fig. 18, Plate II., represent a horizontal plane near the orifice EF through which the fluid of the reservoir MN flows. A fluid particle D situated in this plane, has a motion DB inclined to the axis AB. This motion may be decomposed into two, DC, CB; let us suppose that plane DQ to descend parallel to itself along the axis, with the motion CB; the motion DC of the particle D on the plane DQ, remains to be examined. This motion impresses upon all the particles situated in the plane DQ, a centripetal force towards the centre C.

Let any other horizontal motion whatever, not coincident in direction with DC, be impressed upon the same particles: under the government of these two forces, the particles will describe round the centre C areas proportional to the times, and by the equilibrium of these motions they may assume a horizontal circular rotation.

Let us imagine, that during this horizontal circulation, the particle D, in its approach toward the centre C, as in a spiral, shall describe circular orbits, of which the diameter is successively diminished; let us call the velocity of rotation of the particle $D = v$; its distance from the centre $= r$; the time of one revolution $= t$; and, since the areas must be as the time, we shall have nearly $v = \frac{1}{r}$; $t = r^2$; and the centrifugal force of the particle D will be $= \frac{1}{r^3}$. When we attentively observe the particles which revolve at the surface of the funnel, at MN, we see that the effect which really takes place in nature, is nearly $t = r^2$. Since, therefore the centrifugal force, in approaching the centre C, increases as $\frac{1}{r^3}$ it will become equal to forming an equilibrium against the upper pressure SD, which produces the centripetal force DC. A cavity $KRT'HPV$ will there-

fore be formed, round which the whirling fluid will support itself by the centrifugal force of its rotation.

Let $DQPR$ represent a circular fluid zone, the particles of which turn round the cavity RP, according to the law here indicated. Let the gravity of a fluid particle be $= g$; $CR = a$; $RD = b$; $DX = z$; $XZ = z$; and the velocity of the particle $D = v$. If the centrifugal force of the particle D were equal to its gravity, its velocity, by the theorems of Huyghens, would be equal to that of a body falling by gravity alone, through the space $\frac{a+b}{2}$. And since a heavy body falls in one second through the space of 181 inches $= S$; the velocity of the particle D, on the same supposition, would be $= \sqrt{(2S(a+b))}$. The centrifugal force in the circle is as v^2; the centrifugal force of D will, therefore, really be $= \frac{-v^2 g}{2S(a+b)}$. And since the centrifugal force is $= \frac{1}{r^3}$; taking $\frac{1}{(a+b)^3} : \frac{1}{(a+b-z)^3} = \frac{-v^2 g}{2S(a+b)}$: a fourth term, we shall have the centrifugal force of the element of DX in $X = \frac{v^2 g (a+b)^2 \dot{z}}{2S(a+b-z)^3}$; and that of the filament $DX = A + \frac{v^2 g (a+b)^2}{4S(a+b-z)^2}$. When $z = o$ the fluent is $= o$; whence $A = \frac{-v^2 g}{4S}$. Taking $z = b$, the centrifugal force of the filament DX will be $= \frac{bgv^2}{4a^2 S}(2a+b)$. The quantity bg is the gravity itself of the filament DR. The gravity of this filament is, therefore, to its centrifugal force $= v^2(2a+b) : 4a^2 S$.

When the fluid zone $DRPQ$ is nearer the aperture EF, the pressure SD increases; whence the centrifugal force of the zone must also be increased by diminishing the radius of the cavity KC; hence we may determine the nature of the curve which forms the perpendicular section of the cavity KRT. For greater simplicity, let us suppose that the sides of the vessel have the same form MD as that of the cavity itself, so that

$D\,R = b$ may be constant. Let $A\,C = s$; and $C\,R = y$. Let us substitute y instead of a in the preceding formula. And since the gravity of the filament $D\,R$ is to the gravity of the filament $S\,D = b : x$, we shall have, by composition of ratios, the centrifugal force of the filament $D\,R$, to the pressure $S\,D = bv^2$ $(2y + b) : 4\,x\,y^2\,S$. These quantities must be equal, in order to afford an equilibrium. We have, therefore, $x\,y^2 - \frac{b\,v^2\,y}{2\,S} - \frac{b^2\,v^2}{4\,S}$ $= 0$, for the equation of the curve $K\,R\,T$. This is the sixty-fourth species in the enumeration of lines of the third order, by Sir Isaac Newton. Its convexity is turned towards the axis; it has two asymptotes, one of which is the axis $A\,Y$, and the other is in $M\,N$, supposing the two points $M\,N$ to be infinitely distant.

If the assumed positions in this theory do not absolutely coincide with nature, they approach its effects very nearly. It is not only possible, but there does exist in nature a whirling stream, of which the cavity turns its convex part to the axis, and in which $t = r^2$ very nearly, as is shewn by experiment.

Exper. 26.—Let the orifice $E\,F$ be opened, and any motion whatever be impressed on the fluid, independent of that which its gravity, and the pressure of the circumambient particles tend to produce; the turning immediately begins, and is seen to be more rapid in those parts of the fluid which are nearest the bottom. The cause of this is, that the motion $D\,B$ is more convergent and perceptible in those parts which are nearest the orifice $E\,F$.* The centripetal force $D\,C$ produces its effect there rather than at the upper parts. These last afterwards fall into the cavity which begins to be formed below, by which means they also acquire a centripetal force, and the funnel or cavity opens to a much greater height than that in which the convergence of fluid filaments is observed towards the orifice $E\,F$, in water which is less agitated.

* Bernouilli, Hydrod, sect. 4, § 3. Bossut, art. 427.

EXPER. 27.—Place a floating body at the surface of the fluid, of sufficient magnitude to prevent the formation of the cavity. If the fluid be much agitated, the cavity will take place at the lower part, and air will introduce itself through the opening *E F*. Whence it follows that the pressure of the atmosphere on the upper surface of the fluid is not the cause of the cavity, which assumes the shape of a funnel. The air does not enter, but because it finds an empty space formed by the centrifugal force.

EXPER. 28.—When the fluid remains in a state of tranquillity without eddies, the vessel empties itself in forty seconds; but when the circular motion takes place, the evacuation is accomplished in fifty seconds, more or less. It cannot, therefore, be said in general terms, that the whirling stream absorbs and draws down bodies through the opening *E F*, with more force than if no such circulation took place.

EXPER. 29.—Pour a stratum of oil upon the water of the vessel. As soon as the funnel forms itself, the oil rushes down and issues out before the greatest part of the lower water, upon which it rested. The portions of oil partake less of the rotation of the lower water; having less density, they likewise recede less from the axis than the water; in consequence of which, as they occupy the interior part of the funnel, and are unsupported, they flow out first.

EXPER. 30.—Every other small body which floats on the water in the vessel acts in the same manner as the oil, provided its dimensions be very small. If the volume of the body be somewhat greater, while it approaches the cavity, to fall therein, its extremity, which is nearest the axis, comes into a place where the circulation is more rapid. This rapidity of motion impressed at one extremity of the floating body, is transported, by the laws of mechanics, to its centre of gravity, which is more remote from the axis in a situation where the circular motion

is slower; consequently, the body recedes from the edge of the cavity into which it was about to fall. It returns a short time afterwards, is again repelled, and these alternate motions continue as long as the circumstances which produced them. Lastly, if the body which floats at the surface of the liquor after the funnel has been formed, be of sufficient size to cover the whole cavity, it destroys the funnel in the upper part, and sometimes also in the lower. The reason is, that the body itself cannot turn round its centre but according to the law, $v = r$; it, therefore, destroys by friction the law $v = \frac{1}{r}$ in the parts of the fluid in contact with it, and, consequently, it destroys the funnel itself.

PROPOSITION XII.

The lateral Communication of Motion takes place in the Air as well as in Water.

The stream of air, which moves in the midst of a body of air at rest, produces undulations and eddies round its current in the same manner as in water. These may be observed in the smoke which rises from a furnace, and produces a remarkable aspect, when it issues like a dark tree from an agitated volcano. They may likewise be seen, in the particles which float in an obscure chamber, when a ray of the sun shines in, and the observer blows through them.

If the general wind comes, for example, from the south, it frequently happens that the north side of a mountain is at the same time struck by a north wind. This partial and local wind is nothing but the eddy produced by the mountain itself acting as an obstacle against the principal wind, from the south. It is probably from the same cause, that the wind sometimes

acts in the contrary direction on the sails of a vessel, when they are too obliquely presented to its stream.

The vapour of water which issues from the eolipile carries the surrounding air with it, and drives it against the burning coals opposite to the stream of aqueous vapour. It must not, therefore, be concluded that the aqueous vapour is itself in this case decomposed to maintain the combustion of charcoal.

It is known that the flues of chimneys assist the rising of smoke by their figure; concerning which we have drawn some inductions, in the Seventh Proposition.

In organ-pipes, the air which issues out of the side opening (lumiere) rubs laterally against the extremity of the column of the air included in the pipe. It rubs it on one side in the longitudinal direction, and is, as it were, an elastic file acting upon an elastic surface. Though the column of air be fluid, its parts are, however, so far intermixed together, that the tremulous motion excited at the place of friction is soon communicated laterally through the whole thickness of the column, which receives vibrations of such a kind, that they are an equilibrium with each other, and with the velocity of the stream which affords the friction. For this effect, it is requisite that the column should divide itself at different points or nodes, distributed through the length of the tube.* It is by repeated actions that the wind which issues from the side aperture impresses at length upon the whole column contained in the pipe, a movement of vibration greater than that which the laws of impulse, and of the lateral communication, would permit it to make by a single impulse. In the hautboy, and other similar instruments, having a mouth-piece, or reed, the cause which excites the tremulous motions does not act sideways on the air contained in the pipe; but strikes the column

* Mémoires de l' Acad. an. 1762, page 431.

directly in the middle: for which reason it communicates its vibrations with so much the more effect to the whole mass.

In like circumstances, the force of sound, which is propagated in the atmosphere, depends on the magnitude of the section of the air which is at the extremity of the pipe, and the amplitude of the vibrations of this section. It is this surface which strikes the atmosphere, and communicates the pulsations.* For this reason, conical divergent pipes afford a stronger sound than those which are cylindrical; and these last afford a stronger sound than pipes which are conically divergent. The first cause of the sound which acts at the mouth end of the pipe would never, of itself, excite such strong pulsations in the atmosphere, as it does excite by the lateral communication in the air contained in a divergent conical pipe.

The explanation of this phenomenon may be understood by observing, 1st, That if a number of elastic bodies be disposed in progression, the first will impress upon the last, by the intermedium of the others, more velocity than would be communicated by the immediate stroke: 2. The vibrations excited in the pipe have a certain permanence, which permits them to receive an increase of force by the united effect of successive impulsions: whereas, in the open atmosphere, every pulsation is transient and single.

Is not the augmentation of sound in the speaking trumpet in part owing to the same cause of the lateral communication of motion, rather than to the mere reflection of the sonorous lines from the sides of the tube itself?

I call those *resonant* vibrations, which take place in a tube

* It is known that the material of which a pipe is made does not perceptibly affect the sound. V.

when sound is excited; and I call those *propagated* vibrations, or *pulsations*, which transmit the sound through the atmosphere. I have already pointed out a difference, which appears to me to take place between these two kinds of vibrations; namely, that the former have a certain permanence and connexion with each other, so that each succeeding impulse excites, supports, and reinforces the former; whereas, those pulsations which succeed each other in the atmosphere by the repeated action of the resonant body, are single, and independent of each other.

But the following is a much more remarkable difference between these two kinds of vibrations. When at the extremity of a pipe *A B C*, a resonant vibration is made in the section of air, *B C*, Fig. 2, Plate I., experience shews that this vibration becomes the centre of pulsations propagated all round in *P S Q*. For, on whatever side the observer is placed, whether at *P* or at *Q*, he will hear the sound of the pipe *A B C* nearly as much as at *S*. But when there is no pipe, and the vibration at *C B* is a simple pulsation propagated through the open air from *A* to *B;* in this case the pulsation is not propagated laterally and completely to *P* and *Q*, like the resonant vibration; but is contained almost entirely in the limits *B Z* and *C Y*, with a divergence of between 15 and 20 degrees. This fact has been disputed by various philosophers; but it cannot be questioned, since it is well known that we do not hear the echo, or reflected sound, from the plain surface, unless we place ourselves in the line of reflection, or very near it. If the pulsation of the echo were propagated all round, before the reflecting surface, diverging from thence as a centre, ought we not to hear the echo in every situation whatever, before that reflecting surface? We must, therefore, admit, with regard to sonorous pulsations propagated in the atmosphere, certain exceptions, and even limits, with regard to the lateral communication of motion which we have pointed out in the first proposition, and in the fifth, with regard to water.

Addition respecting the contracted Vein.

Much has been written respecting the convergent directions assumed by the particles of a fluid contained in a vessel, previous to their being emitted through an aperture in the side of the vessel itself, and concerning the form of the contracted vein which is thus produced. The reflections and experiments which I shall proceed to give, may afford some further explanation in this respect.

I shall begin by defending the fundamental doctrine of hydraulics against the opinion of a learned man, distinguished by his labours and his zeal for the advancement of science—Lorgna, the founder of the Italian Society. He pretends* that the contracted vein is nothing else but the continuation of the Newtonian cataract, and that the celerity of the fluid issuing from an orifice in a thin plate, is much less than that of a body which falls from the height of the charge.

Let MD, Fig. 22, Plate II., represent the axis of the vein which issues from B. The radius of the circular orifice $BC = BD = 1$; $MB = a$. Lorgna pretends that $0{\cdot}472\,a = HB$, is the height which would produce, in any heavy body, the velocity of efflux in BC. He supports this proposition by computations deduced from the mutual action of the particles of the fluid contained in the vessel. But, after having seen the failure of the efforts of the greatest geometers on this very subject, we ought to mistrust all these demonstrations founded on mechanical principles, very true in themselves, but of which the application to an infinity of bodies, which move and are pressed in every direction, becomes extremely difficult, if not impossible. Let us see whether the theory of Lorgna agrees with experiment. Supposing the velocity of the fluid at B arising

* Mem. della Società Italiana, vol. iv.

from the elevation $HB = 0{\cdot}472\ a$, the velocity of the same fluid in D will be increased in the ratio of $\sqrt{HB} : \sqrt{HD}$; and the vein in D will be contracted in the same ratio. Whence $DE = \left(\frac{0{\cdot}472\ a}{1+0{\cdot}472\ a}\right)^{\frac{1}{4}}$; which is the formula of the hyperbolic conoid of Newton. If this be the sole cause of the contraction, the dimensions of DE ought, very nearly, to agree with this figure when examined by experiment. But they, in reality, differ from it very much, as may be seen in the following table.

Authors of the Experiments.	Value of DE found by actual Measurement.	Value of DE calculated by the preceding Formula.
Poleni (de Castellis, § 35)....	0·79	0·97
Michelotti; Sperim. Idraul. Tom. I. Exper. 46; Tom. II. Exper. 4	0·80	0·99
Bossut (Hydrodyn. art. 437. Exper. 5.)	0·818	0·99
Myself, with 35 inches charge and a horizontal circular orifice of 18 lines in diameter.	0·798	0·984

It is evident, that the contraction of the vein, as found by experiment, is incomparably greater than can be produced by the acceleration of gravity, even in descending streams. But what can we say of horizontal and ascending jets, in which, assuredly, the acceleration of gravity does not take place, but in which, nevertheless, the contraction is observed nearly in the same manner as in descending currents? The contraction of the stream is, therefore, very different from the Newtonian hyperboloid.

Desirous of proving that the vein does not possess the whole velocity arising from the height of the fluid above the centre of

the orifice, Lorgna relates the experiments of Kraft,* which are not applicable to the question, because they were made with cylindrical pipes, and we have seen that such pipes always destroy part of the velocity of the fluid; consequently, we cannot establish any rule from them which shall apply to orifices through thin plates.† He wishes not to determine the velocity of ascending jets by the height to which they rise, because he is apprehensive that the preceding part of the stream or jet is urged and supported by the succeeding part nearly to the height of the charge. Nevertheless, if we interrupt the jet all at once, the last portions of water fly to the same height as those which preceded them, without having any continued column of the fluid below to follow and support them: these last portions must consequently have received, at their passage through the orifice, all the velocity which was necessary to raise them nearly to the surface of the fluid in the reservoir.

Let us confine ourselves, if it be thought proper, to horizontal jets; the experiment which I have related, as a term of comparison, appears to me to be decisive. Under the charge of 32·5 inches, the vertical line PM, Fig. 1, Plate I., being 54 inches, the horizontal line MN was always 81·5 inches, which was only two inches less than it would have been if the jet had preserved in the direction of the horizon all the velocity which a heavy body acquires in falling from the height of 32·5 inches. The diameter of the contracted vein was 14·3 lines very nearly. Since the quantity of 81·5 inches in MN supposes in the contracted vein a velocity of 149·5 inches per second; this number, multiplied by the area of the contracted vein itself, gives the expenditure of four cubic feet in forty-one seconds of time, which is also the result of experiment. We have, therefore,

* Acta Petrop. vol. viii.

† Torricelli took notice of this difference at page 168 of his Works, "quotiescunque autem aqua per tubum latentem decurrens per angustias transire debuerit, falsa omnia reperies."

three measures determined by experiment, which agree, and mutually confirm each other; namely, the quantity MN, the contraction of the stream, and the time of expenditure. And since the quantities observed by Bossut, Michelotti, and Poleni, give nearly the same results, it can no longer be doubted, 1. That the contraction of the stream is nearly 0·64 of the orifice; 2. That the velocity of the contracted vein is nearly the same as that of a heavy body which may have fallen through the height of the charge.

These two experimental principles are true in all cases where the orifice is considerably small in proportion to the section of the reservoir, where that orifice is made through a thin plate, and the internal afflux of the fluid filaments is made in an uniform manner round the orifice itself. But what would be the consequence if this internal afflux should be modified in a manner different from what usually happens? The following experiments were made with the intention of ascertaining some of the most remarkable effects of these particular modifications in the direction of the fluid filaments which press each other in order to pass through the orifice.

Exper. 31.—In the orifice $ACBD$, Fig. 21, Plate II., the two sides AB are parallel to the horizon; the extremities CD are rounded; the width of this aperture is less than two lines, its length eighteen lines, and the charge 32·5 inches. The section of the stream which issues from this orifice, first assumes the form EF; after which, the two extremities EF approaching nearer and nearer to enlarge the middle part of the section of the stream, at 4·5 inches distance from the orifice, acquire the quadrangular form GH. The stream afterwards extends itself in the perpendicular direction in the form of a large fan KL.

I have repeated the experiment by placing the longitudinal axis of the orifice CD vertically. In this case, the same

phenomena were produced; EF becoming vertical, and KL horizontal, both preserving their form.

The fluid filaments, which, issuing out of the orifice, pass near the two opposite borders A, B, are very near each other; and being convergent, they tend to unite at a very short distance from the orifice. The filaments $C\,D$ are more remote, and, perhaps, less convergent; they cannot unite but at a greater distance than the two former. In this case, therefore, there are movements which tend to form two contractions, the one nearer, and the other more remote from the orifice; these two contractions counterbalance each other in part. Their mutual opposition carries the effect $G\,H$ to a distance five times greater than that of the contracted vein of a circular orifice, having a diameter of the same breadth as that of this orifice.

In this experiment we see the cause of a phenomenon which has been observed in some particular cases by Poleni and others, without giving the explanation. In every orifice of a right-lined figure through a thin plate, the angles of the contracted vein answer to the sides of the orifice, and the contrary. When the quadrangle orifice has the situation $M\,N\,O\,P$, the greatest contraction of the stream is made at a greater distance than in a circular aperture; it assumes the form and situation $Q\,R\,S\,T$. The reason is, that the opposite angles $M\,P$ are more remote from each other than the side I, V, whence the same thing happens as in the long orifice $A\,C\,B\,D$. In the same manner, the triangular orifice in the situation X produces a contraction of the form and in the situation Z, &c.

Exper. 32.—The orifice being the horizontal cleft $C\,D$, Fig. 21, the place $G\,H$, or most contracted point of the stream, was found to be distant from that orifice, as in the following table:

Height of the charge above the orifice *C D*.	Distance of the greatest contraction *GH*.
Inches.	*Lines.*
32·5	53
18	48
10	40
6	36

The long orifice *C D* exhibits to us, under an enlarged dimension, the distance of the contracted vein from the orifice. By this means the foregoing table shews us, in a very sensible manner, that the contraction of the stream takes place at a greater distance, under strong charges, than in those which have but little elevation.

Exper. 33.—To the centre of the circular orifice *A B*, Fig. 23, formed in a thin plate, I disposed within the reservoir, the cone of metal *D G E*, with a cylindrical part *C F G D*, in such a manner that the cone was movable along its own axis *I V*, and its summit *E* could be protruded, more or less, through the orifice *A B*, approaching or receding from the point *V*. The measures in lines were $AB = 18$; $IE = 24$; $DG = 27$; $CD = 8$. This apparatus was applied to the orifice *P*, Fig. 1, Plate I.; the charge being 32·5 inches. The results were as in the following table :

Quantity *E X*, by which the summit of the cone projects beyond the line *A B*.	Distance of the contracted vein.	Distance of *M N*.	Time of expenditure of four cubic feet.
Lines.	*Lines.*	*Inches.*	
11·1	9·1	76	85″
6·6	12·3	77·5	53
0	14	78·5	43
The cone removed.	14·3	81·5	41

I intend to repeat and vary this experiment, in order to discover the cause of the singular phenomenon which it presents.

EXPER. 34. — The orifice being a semicircle, Plate II., Fig. 24, having the diameter $A\,B$, 11·2 lines, I applied within the vessel a plane $Q\,A\,B$, perpendicular to the plate in which the orifice was made. The line $A\,B$ was perpendicular to the horizon, and the charge 32·5 inches. The jet deviated in the horizontal direction in $P\,F\,G$, departing from the axis $C\,E$ towards the side on which the plane $Q\,P$ was placed. The angle $F\,C\,E$ was 9° 5′, and the angle $F\,C\,G$ was 36°. The vertical section of the jet had the form $K\,L$, so that the largest part of the jet was in F. The four cubical feet of water issued out in 206 seconds.

The results of this experiment are analogous to those of the experiments 31 and 33.

EXPER. 35. — Citizen Borda, in a very interesting memoir,* relates a peculiar phenomenon, of which he has given a very simple demonstration, from the principle of the equality of pressure which fluids exert in every direction. It is, that if the extremity of a cylindrical tube be pushed into the interior part of the reservoir, the contraction of the vein is greater, and the expenditure less, than if the same tube be applied to the side of the vessel. I have repeated this experiment, and observed a similar result when the tube was cylindrical from one end to the other, like that made use of by the author, and when the water was made to flow out in a full stream. I afterwards gave to the interior extremities of the pipe the form $A\,C$, Fig. 4, Plate I. of the common contracted vein; in this case, there was no longer any remarkable difference between the two expenditures, in the two situations of the tube. For when the end $A\,C$ was pushed into the interior part of the reservoir, the

full tube afforded, in eighty-one seconds, the same quantity of water as was furnished in eighty seconds, when it was applied to the side of the vessel. It may be presumed, that if the part *A C* had more perfectly possessed the form of the contracted vein, the slight difference of one second would have disappeared.

A

SUMMARY

OF THE MOST USEFUL PARTS OF

HYDRAULICS;

CHIEFLY EXTRACTED AND ABRIDGED FROM

EYTELWEIN'S HANDBUCH DER MECHANIK
UND DER HYDRAULIK.

By THOMAS YOUNG, M.D., F.R.S.

&c. &c. &c.

INTRODUCTION.

In the practice of a Civil Engineer, much of his success depends on a sound knowledge of the principles of the motion of fluids, and of the circumstances attending such motions; at the same time, few have leisure to enter into such inquiries, unless they be brought before them in a condensed form, with clear directions for their immediate application to practice.

A work of this nature having been published in Germany by Professor Eytelwein, an abstract or summary of its contents was drawn up by Dr. Thomas Young, with such modifications and improvements as he considered would add to its value to an English reader. This summary was first published in the Journals of the Royal Institution in 1802, and the importance of so comprehensive a view of the doctrine of hydraulics, being immediately connected with the preceding experimental papers, was so evident, that we solicited Dr. Young's permission to republish it, and it was granted in the most kind and liberal manner; but, in consequence of his not having there

distinguished the extent to which we are indebted to Dubuat for forming the theorems given by Eytelwein, Dr. Young was desirous to add the following passage from his article, Hydraulics, in Napier's "Supplement to the Encyclopædia Britannica :"—"It has happened, from a combination of accidental circumstances, that Dubuat has been deprived of a considerable portion of his just merits, in favour of Mr. Eytelwein, without any kind of voluntary plagiarism on the part of that very respectable professor. The author of the English abstract of his work was acquainted with Dubuat's book, only from having read the extracts copied from it by Professor Robison, in his contributions to the Encyclopædia Britannica, and he was hence led into the mistake of supposing Professor Eytelwein the author of many improvements, which he had no more idea of claiming than he could have had of several modifications introduced into the 'Summary of Hydraulics,' chiefly extracted from his book, which appeared in the Journal of the Royal Institution, and which have been quoted as Eytelwein's."

To this Summary we have added a few notes, and given the rules in words at length in those instances where Dr. Young had not done so; and we have also added references to the experimental papers of Smeaton and Venturi.—EDITOR.

A

SUMMARY

OF THE MOST USEFUL PARTS

OF

HYDRAULICS.

The theory of hydraulics has never been carried to a very high degree of perfection upon mathematical foundations alone; nor has it hitherto, even with the assistance of experiment, been rendered of much practical utility. Newton began the investigation of the motion of fluids. Daniel Bernoulli added to Newton's propositions much valuable matter, both from calculation and from experiment. D'Alembert and many later authors have exercised their analytical talents in inquiries of a similar nature. But another and a more practical mode of attaining hydraulic knowledge has been attempted by a distinct class of investigators, at the head of whom stands Chevalier Dubuat. These have begun from experiment alone, and have laboriously deduced, from very ample observations of the actual results of various particular cases, the general laws by which the phenomena appear to be regulated, or, at least, the formulas by which the effect of new combinations may be predicted. But it must be confessed that these formulas, however accurate, are too intricate to be retained in the memory, or to be very easily applied to calculations from particular data.

Mr. Eytelwein, a gentleman already known to the public by his translation of Dubuat's work into German, with important additions of his own, and honoured with several employments and titles relative to the public architecture of the Prussian dominions, has collected into this compendium of mechanics and hydraulics the principal facts that have been ascertained, as well by his own experiments as by those of former authors, especially such as are the most capable of practical application; and he appears to have done this in so judicious a manner, as to make his book a most valuable abstract of every thing that can be deduced from theory respecting natural and artificial hydraulics. This elegant conciseness deserves so much the more praise, as his countrymen in general appear too often to make a merit of prolixity; and we shall have occasion to remark, that, besides the convenience of simplicity, he has sometimes been fortunate enough to unite with it the advantage of superior accuracy.

The first part of the work is but short; it relates to proper mechanics, and has little that is remarkably new or interesting. In treating of pendulums, the author informs us, with reference to another work of his own, that the Rhinland or Brandenburg foot contains 139·13 French lines. Hence it appears that 100 Rhinland feet are exactly 103 English; and in this paper the measures will be reduced accordingly.

The second part, relative to hydraulics, contains, besides a short Introduction, twenty-four chapters, almost every one of which presents to us something of importance.

CHAPTER I.

Of the Motion of Water flowing out of Reservoirs, and of the Contraction of the Stream.

The velocity of water, flowing out of a horizontal aperture, is as the square root of the height of the head of the water.

That is, the pressure, and, consequently, the height, is as the square of the velocity; for the quantity flowing out in any short time is as the velocity; and the force required to produce a velocity in a certain quantity of matter, in a given time, is also as that velocity; therefore, the force must be as the square of the velocity. The proposition is fully confirmed by Bossut's experiments: the proportional velocities, with a pressure of 1, 4, and 9 feet, being 2722, 5436, and 8135, instead of 2722, 5444, and 8166; a very inconsiderable difference.

There is another mode of considering this proposition, not mentioned by Eytelwein, which is a very good approximation. Supposing a very small cylindrical plate of water immediately over the orifice to be put in motion at each instant, by means of the pressure of the whole cylinder standing on it, and supposing all the gravitation of the column to be employed in generating the velocity of the small cylindrical plate, neglecting its own motion; this plate would be urged by a force as much greater than its own weight as the column is higher than itself; and this, through a space shorter in the same proportion than the height of the column. But where the forces are inversely as the spaces described, the final velocities are equal. Therefore, the velocity of the water flowing out must be equal to that of a heavy body falling from the height of the head of water,

which is found very nearly by multiplying the square root of that height in feet by 8, for the number of feet described in a second. Thus, a head of 1 foot gives 8; a head of 9 feet, 24.

The well-known circumstance of the *contraction* of the stream or vein of water, running out of a simple orifice in a thin plate, reduces the area of its section at the distance of about half its diameter from the orifice, from 1 to 0·660, or 0·666, according to Bossut; to 0·631, according to Venturi;* and to 0·64, or $\frac{16}{25}$ according to the author's own experiment: hence the diameter is reduced to $\frac{4}{5}$.

The quantity of water discharged is very nearly, but not quite, sufficient to fill this section with the velocity due, or corresponding to the height; for, finding more accurately the quantity discharged, the orifice must be supposed to be diminished to 0·619, or nearly $\frac{5}{8}$. Hence, we may multiply the square root of the height by 5, instead of 8, for this mean velocity in a simple orifice.

If we apply the shortest pipe that will cause the stream to adhere every where to its sides, which will require its length to be twice its diameter, the discharge will be about $\frac{13}{16}$ of the full quantity, and the velocity may be found by taking $6\frac{1}{2}$ for a multiplier.

The greatest diminution is produced by inserting a pipe so as to project within the reservoir, probably because of the greater interference of the motions of the particles approaching its orifice in all directions; in this case the discharge is reduced nearly to a half.

* See Venturi's Experiments, p. 133.

A conical tube, approaching to the figure of the contraction of the stream, procured a discharge of 0·92; and when its edges were rounded off, of 0·98, calculating on its least section.

Venturi has asserted that the discharge of a cylindrical pipe may be increased by the addition of a conical tube, nearly in the ratio of 5 to 2;* but Mr. Eytelwein finds this assertion somewhat too strong, and observes, that when the pipe is already very long, scarcely any effect is produced by the addition of such a tube. He proceeds to describe a number of experiments made with different pipes, where the standard of comparison is the time of filling a given vessel out of a large reservoir, which was not kept always full, as it was difficult to avoid agitation in replenishing it, and this circumstance was perfectly indifferent to the results of the experiments. They confirm the assertion, that a compound conical pipe may increase the discharge to twice and a half as much as through a simple orifice, or to more than half as much more as would fill the whole section with the velocity due to the height; but where a considerable length of pipe intervenes, the additional tube appears to have little or no effect.

The chapter concludes with a general table of the coefficients for finding the mean velocity of the water discharged by the pressure of a given head under different circumstances.

For the whole velocity due to the height, the coefficient, by which its square root is to be multiplied, is 8·0458.

For an orifice of the form of the contracted stream, 7·8.

For wide openings, of which the bottom is on a level with that of the reservoir; for sluices with walls in a line with the

* The ratio assigned by Venturi is only as 24 is to 12·1, or nearly as 4 is to 2.—See Venturi's Experiments, Prop. VII., p. 154; but these experiments were not likely to afford a rule for any other than the particular case that was tried.—Ed.

orifice ; and for bridges with pointed piers, 7·7. For narrow openings, of which the bottom is on a level with that of the reservoir, for smaller openings in a sluice with side walls, for abrupt projections and square piers of bridges, 6·9.

For short pipes, from two to four times as long as their diameter, 6·6.

For openings in sluices without side walls, 5·1.

For orifices in a thin plate, 5.

CHAPTER II.

Of the Discharge of Water by horizontal and by small lateral Orifices in a Vessel continuing full.

The principles detailed in the first chapter are here applied to particular cases.

CHAPTER III.

Of the Discharge by rectangular Orifices in the side of the Reservoir, extending to the Surface.

The velocity varying nearly as the square root of the height, may here be represented by the ordinates of a parabola, and the quantity of water discharged by the area of the parabola, or two-thirds of that of the circumscribing rectangle. So that the

quantity of water discharged may be found by taking two-thirds of the velocity due to the mean height, and allowing for the contraction according to the form of the opening, as explained in the first chapter.*

The author has found this mode of calculation sufficiently near to the results of Dubuat's experiments, and to some accurate observations of his own.

He proposes, for example, a lake in which a rectangular opening is made without any oblique lateral walls, 3 feet wide, and extending 2 feet below the surface of the water. Here the coefficient of the velocity, corrected for contraction, is 5·1, and the corrected mean velocity, $\frac{2}{3}\sqrt{2} \times 5{\cdot}1 = 4{\cdot}8$; therefore, the area being 6, the discharge of water in a second is 28.8 cubic feet, or nearly four hogsheads.

The same coefficient serves for determining the discharge over a weir of considerable breadth; and hence it is easy to deduce the depth or breadth requisite for the discharge of a given quantity of water. For example, a lake has a weir 3 feet in breadth, and the surface of the water stands at the height of 5 feet above it: it is required how much the weir must be widened in order that the water may be a foot lower. Here the velocity is $\frac{2}{3}\sqrt{5} \times 5{\cdot}1$, and the quantity of water $\frac{2}{3}\sqrt{5} \times 5{\cdot}1 \times 3 \times 5$; but the velocity must be reduced to $\frac{2}{3}\sqrt{4} \times 5{\cdot}1$, and then the section will be $\frac{\frac{2}{3}\sqrt{5} \times 5{\cdot}1 \times 3 \times 5}{\frac{2}{3}\sqrt{4} \times 5{\cdot}1} = \frac{\sqrt{5} \times 3 \times 5}{\sqrt{4}}$ $= 7{\cdot}5 \times \sqrt{5}$; and the height being 4, the breadth must be $\frac{7{\cdot}5}{4}\sqrt{5} = 4{\cdot}19$ feet.

* The depth of the stream at the edge of the waste board is about one-half the depth of the edge below the surface of the water, when it is only a thin waste board. Dr. Robison mentions some experiments in which it was two-sevenths; but it varies as the waste board is made thicker.—Ed.

CHAPTER IV.

Of the Discharge from Reservoirs with lateral Orifices of considerable Magnitude, with a constant Head of Water.

This may be found by determining the difference in the discharge by two open orifices of different heights: but, in most cases, the problem may be solved with nearly equal accuracy, by considering the velocity due to the distance of the centre of gravity of the orifice below the surface.

CHAPTER V.

Of the Discharge from Reservoirs receiving no Supply of Water.

For prismatic vessels, all the particulars of the discharge may be calculated from the general law, that twice as much would be discharged from the same orifice if the vessel were kept full during the time which is required for its emptying itself. (Dr. Young's Lectures, II. 61.) Where the form is less simple, the calculations become intricate, and are of little importance.

CHAPTER VI.

Of the Discharge from compound or divided Reservoirs.

The author observes from Buat, that the discharge through an orifice between two reservoirs, below the surface, is the same as if the water ran into the open air. Hence he calculates the discharge when the water has to pass through several orifices in the sides of as many reservoirs open above. In such cases, where the orifices are small, the velocity in each may be considered as generated by the difference of the heights in the two contiguous reservoirs, and the square root of the difference will, therefore, represent the velocity; which must be in the several orifices, inversely as their respective areas; so that we may calculate from hence the heights in the different reservoirs when the orifices are given. Mr. Eytelwein then considers the case of a lock which is filled from a canal of an invariable height, and determines the time required, by comparing it with that of a vessel emptying itself by the pressure of the water that it contains, observing that the motion is retarded in both cases in a similar manner, and he finds the calculation agree sufficiently well with experiments made on a large scale. The motion of water through different compartments of a closed cavity is also determined.

CHAPTER VII.

Of the Motion of Water in Rivers.

The simple theorem by which the velocity of a river is determined, appears to be the most valuable of Mr. Eytelwein's improvements; although the reasoning from which it is deduced is somewhat exceptionable. The friction is nearly as the square of the velocity; not because a number of particles proportional to the velocity are torn asunder in a time proportionally short, for, according to the analogy of solid bodies, no more force is destroyed by friction when the motion is rapid than when slow, but because, when a body is moving in lines of a given curvature, the deflecting forces are as the squares of the velocities, and the particles of water in contact with the sides and bottom must be deflected, in consequence of the minute irregularities of the surfaces on which they slide, nearly into the same curvilinear path, whatever their velocity may be.

At any rate, we may safely set out with the hypothesis, that the principal part of the friction is as the square of the velocity. And the friction is nearly the same at all depths; for Professor Robison found that the time of the oscillation of the fluid in a bent tube was not increased by increasing the pressure against the sides, being nearly the same when the principal part of the tube was situated horizontally as when vertically. The friction will, however, vary according to the surface of the fluid which is in contact with the solid, in proportion to the whole quantity of fluid: that is, the friction for any given quantity of water will be as the surface of the bottom and sides of a river directly, and as the whole quantity of water in the river inversely; or supposing the whole quantity of water to be spread on a horizontal surface, equal to the bottom and sides, the friction is inversely

as the height at which the river would then stand, which is called the *hydraulic mean depth*.

Now, when a river flows with an uniform motion, and is neither accelerated nor retarded by the action of gravitation, it is obvious that the whole weight of the water must be employed in overcoming this friction; and, if the inclination vary, the relative weight, or the force that urges the particles along the inclined plane, will vary as the height of the plane when the length is given, or as the fall in a given distance; consequently, the friction, which is equal to the relative weight, must vary as the fall, and the velocity, which is as the square root of the friction, must be as the square root of the fall; and, supposing the hydraulic mean depth to be increased or diminished, the inclination remaining the same, the friction would be diminished or increased in the same ratio; and, therefore, in order to preserve its equality with the relative weight, it must be proportionally increased or diminished by increasing the square of the velocity, in the ratio of the hydraulic mean depth, or the velocity in the ratio of its square root. We may, therefore, expect that the velocities will be conjointly as the square root of the hydraulic mean depth, and of the fall in a given distance, or as a mean proportional between these two lines. Taking two English miles for a given length, we must find a mean proportional between the hydraulic mean depth and the fall in two miles, and inquire what relation this bears to the velocity in a particular case, and thence we may expect to determine it in any other. According to Mr. Eytelwein's formula, this mean proportional is $\frac{11}{10}$ of the velocity in a second.

In order to examine the accuracy of this rule, we may take an example which could not have been known to Mr. Eytelwein. Mr. Watt observed, as Professor Robison informs us, in the article "River" of the Encyclopædia Britannica, that in a canal 18 feet wide above, and 7 below, and 4 feet deep, having a fall of four inches in a mile, the velocity was 17 inches in a second at the surface, 14 in the middle, and 10 at the bottom;

so that the mean velocity may be called 14 inches, or somewhat less, in a second. Now, to find the hydraulic mean depth, we must divide the area of the section, $2\,(18 + 7) = 50$, by the breadth of the bottom and length of the sloping sides added together, whence we have $\frac{50}{20{\cdot}6}$, or 29·13 inches: and the fall in two miles being 8 inches, we have $\sqrt{(8 \times 29{\cdot}13)} = 15{\cdot}26$ for the mean proportional, of which $\frac{10}{11}$ is 13·9; agreeing exactly with Mr. Watt's observation. Professor Robison has deduced from Dubuat's elaborate theorems 12·568 inches for the velocity, which is considerably less accurate.

For another example, we may take the Po, which falls 1 foot in two miles, where its mean depth is 29 feet; and its velocity is observed to be about 55 inches in a second. Our rule gives 58, which is, perhaps, as near as the degree of accuracy of the data will allow.

On the whole, we have ample reason to be satisfied with the unexpected coincidence of so simple a theorem with observation: and, in order to find the velocity of a river from its fall, or the fall from its velocity, we have only to recollect that the velocity in a second is $\frac{10}{11}$ of a mean proportional between the hydraulic mean depth and the fall in two English miles. This is, however, only true of a straight river flowing through an equable channel.

For the slope of the banks of a river or canal, Mr. Eytelwein recommends that the breadth at the bottom should be $\frac{2}{3}$ of the depth, and at the surface $\frac{1}{3}^0$: the banks will then be in general capable of retaining their form.* The area of such a section is twice the square of the depth, and the hydraulic mean

* When a canal has the proportions described in the text, the slope of the bank within the water makes an angle of 37° with the horizon, or of 4 to 3; the slope usually employed in this country is 3 to 2, making an angle of about 34°; and the relation between the breadth and depth varies considerably according to the nature of the traffic, and of the canal-boats.—Ed.

depth $\frac{2}{3}$ of the actual depth. He then investigates the discharge of a canal of which the bottom is horizontal. The velocity appears, in this case, to be somewhat greater than in a similar canal of which the bottom is parallel to the surface.

The author remarks that the velocity is greater near the concave than the convex side of a flexure: a circumstance probably occasioned by the centrifugal force accumulating the water on that side.* No general rule can be given for the decrease of the velocity in going downwards: but sometimes the maximum appears to be a little below the surface. In the Arno the velocities are, at 2 feet below the surface, 39½ inches; at 4, 38½; at 8, 37; at 16, 33½; at 17, 31. In the Rhine, at 1 foot, 58 inches; at 5, 56; at 10, 52; at 15, 43. As an approximation to the mean velocity, the author directs us to deduct from the superficial velocity $\frac{1}{130}$ for every foot of the whole depth. For instance, if the depth were 13 feet, and the superficial velocity 5 feet, to take 4½ as the average velocity of the whole river. This can only, however, be true in large rivers; for, in the canal measured by Mr. Watt, the superficial velocity must be diminished nearly $\frac{1}{5}$ for a depth of only 4 feet. And we may, in general, come quite as near to the mean velocity by taking $\frac{9}{10}$ of the superficial velocity; although this may still differ materially from the true medium. But, comparing this with the former theorem for the velocity, which gives a result oftener above than below the truth, we may bring them both into a form easily recollected, thus:

The superficial velocity of a river is nearly a mean proportional between the hydraulic mean depth, and the fall in two miles; and the mean velocity of the whole water is, still more nearly nine-tenths of this mean proportional.†

* When the direction of a current is changed by a bend in its channel, the portion of the stream most distant from the centre of curvature is sensibly of a higher level than that which is nearest to the centre.—Ed.

† The inclination of the surface of rivers is of more importance than is gene-

We may find a double confirmation of these principles in Major Rennel's account of the Ganges (Phil. Trans. 1781, p. 87). He informs us, that, "at 500 miles from the sea, the channel is 30 feet deep when the river is at its lowest; and it continues at least this depth to the sea;" that "a section of the ground, parallel to one of its branches, in length 60 miles, was taken by order of Mr. Hastings;" that "the windings of the river were so great as to reduce the declivity on which the water ran, to less than 4 inches per mile;" that "the medium rate of the motion of the Ganges is less than three miles an hour in the dry months;" that is, its superficial velocity. Now, allowing a little for the banks, or shelving sides, we may take exactly 30 feet as the hydraulic mean depth; then, if the fall in two miles were precisely $\frac{2}{3}$, we should have $\frac{2}{3} \times 30 = 20$; and $\sqrt{20} = 4{\cdot}47$ for the velocity in a second, or 3·05 miles in the hour: which is a little greater than the observed velocity, because the fall was assumed somewhat too great.

Again (p. 110), "The river, when full, has thrice the volume of water in it, and its motion is accelerated in the proportion of 5 to 3. We may assume, that the hydraulic mean depth is doubled at the time of the inundation, whence the velocity will be increased in the ratio of 7 to 5: but the inclination of the surface is probably somewhat increased at the same time, which may easily be supposed to increase the velocity still further, from 1·4 to 1·7."*

rally imagined. In questions relating to the right of water for mills, many unjust decisions have been made from want of attention to it. The fall, hydraulic mean depth, and velocity at the time of examination may be easily ascertained, and from ascertaining the quantity, the hydraulic mean depths would be increased or diminished by an increase or diminution of the quantity of water; the inclination of the surface corresponding to the quantity of water in the river at any time may be nearly ascertained by the methods described in this chapter. —Ed.

* The lateral communication of motion in rivers, and effect of eddies, are treated of by Venturi, in Prop. X. p. 165.—Ed.

CHAPTER VIII.

Of the Discharge and the Swell in the case of Weirs, Falls, and Contractions, in Rivers and Canals.

The methods employed in the third chapter require here some modification, since the water arrives at the place of descent with a considerable velocity; and it is evident, from mechanical as well as from hydraulic considerations, that the ultimate velocity will exceed that which is due to the depth of the stream at the place of its descent, and that it will correspond to a height equal to the sum of the heights capable of producing these velocities. Hence, we may calculate the effect of a bar in elevating the surface of a river; how broad a weir must be, in order to produce a certain elevation; and how much water will run over a given weir, according to collateral circumstances. When a bar is below the level of the lower water, we must consider the difference of the two levels as constituting the fall, the whole of the stream below the level of the lower water deriving its additional velocity from this difference only.

The extent of the swell produced by a given elevation of the surface of a river in consequence of the effect of a weir or bar, may be determined by calculating, from the rules for finding the velocity of rivers, the inclination necessary for producing a given discharge; the depth being greater, the inclination immediately above the bar will be less, but the effect of the swell does not terminate at the point where the new surface, if straight, would have met the original surface; for, on account of the rounding of this angle, it extends nearly twice as far. The effect of reducing the breadth of a river may be determined in a manner nearly similar. The author remarks, that a consider-

able diminution of breadth produces but a small elevation, a result which appears to be conformable to experience; but that where depth is required for navigation, it may often be obtained by a projection built out from the bank, which may be sufficient to increase the river's velocity, and to cause it to excavate its bed.

CHAPTER IX.

Of the Motion of Water in Pipes.

The author has attempted to simplify this subject nearly in the same manner as that of the motion of rivers, and apparently with considerable success. He observes, that the head of water may be divided into two parts, one of which is employed in producing velocity, the other in overcoming the friction: that the height employed in overcoming the friction must be as the length of the pipe directly, and also directly as the circumference of the section, or as the diameter of the pipe, and inversely as the content of the section, or as the square of the diameter; that is, on the whole inversely as the diameter; this height, too, must vary like the friction, as the square of the velocity.

Hence, $f = \frac{a l v^2}{d}$; f denoting the height due to the friction, and a a constant quantity: therefore, $v^2 = \frac{f d}{a l}$. Now, the height employed on the friction corresponds to the difference between the actual velocity and the actual height, or $f = h - \frac{v^2}{b^2}$; where b is the coefficient for determining the velocity from the height; consequently, $v^2 = \frac{b^2 d h - d v^2}{a b^2 l}$; and $v^2 = \frac{b^2 d h}{a b^2 l + d}$ Now $b = 6{\cdot}6$, and from Buat's experiments, $a b^2$ is determined to be $\cdot 0211$, which agrees the more accurately where the velocity

is between 6 and 24 inches in a second. Whence we have $v^2 = \frac{43{\cdot}6dh}{{\cdot}0211l+d}$; or $v = 45{\cdot}5\sqrt{\left(\frac{dh}{l+47d}\right)}$; but it is somewhat more accurate to make $v = 50\sqrt{\left(\frac{dh}{l+50d}\right)}$; all the measures being expressed in English feet.*

* From this equation some exceedingly useful practical rules may be derived. In its present shape, it only shews the velocity of water flowing through pipes; and is equivalent to the following rule:—

To determine the velocity of discharge of a pipe, when the height of the water in the reservoir above the point of discharge, and the length and diameter of the pipe, are given.

RULE.—*Multiply 2500 times the diameter of the pipe, in feet, by the height in feet, and divide the product by the length in feet, added to 50 times the diameter, then the square root of the quotient will be the velocity of discharge in feet per second.*

Example.—Let the diameter of the pipe be ·375 feet; the height of the water in the reservoir above the point of discharge 51·5 feet, and the length of pipe 14637 feet. Here, $\frac{2500 \times {\cdot}375 \times 51{\cdot}5}{14637 + (50 \times {\cdot}375)} = \frac{48281{\cdot}25}{14655{\cdot}75} = 3{\cdot}3$ very nearly, and the square root of 3·3 is 1·816; hence the velocity is 1·816 feet per second. These are the measures of one of the Edinburgh water pipes, described in Smeaton's Reports (vol. iii. p. 231, first edition), and the actual velocity, it appears, was 1·815 feet per second.

To determine the quantity of water a pipe will discharge, when the height of the reservoir above the place of discharge, the length of the pipe, and the diameter, are known, multiply the area of the pipe in feet by the velocity in feet, as found by the above rule, and the result will be the discharge in cubic feet per second.

If from these rules the equation for the quantity discharged be formed, and that quantity be called Q, we have, $d\left(\frac{Q^2(l+50d)}{1542h}\right)^{\frac{1}{5}}$; from whence the diameter of the pipe to supply a given discharge may be found; or more easily, $\log. d = \frac{1}{5}(-2{\cdot}6515 + 2 \log. Q + \log.(l+50d) - \log. h;)$ when d is in inches and Q in cubic feet per minute, which is as follows, in words.

To find the diameter of a pipe to supply a given quantity of water per minute, the height of the surface in the reservoir above the place of discharge, and the length of the pipe, being known.

RULE.—*Add together the constant logarithm* — 2·651500, *twice the log. of the quantity to be discharged in cubic feet per minute, and the log. of the length in feet. From the sum subtract the log. of the height of the reservoir in feet, and one fifth of the remainder is the log. of the diameter of the pipes in inches nearly.*

When the pipe is bent into angles, or rather arcs, we must diminish the velocity thus found, by taking the product of its

To obtain a more accurate result, repeat the operation; only, instead of taking the log. of the length only, let 4·2 times the diameter in inches found by the rule be added to the length, and take the log. of the sum instead of that of the length, and proceed as before.

Example.—A reservoir for the service of a town is to be supplied at the rate of 12 cubic feet per minute, the length of the pipe being 14637 feet, and the fall 51·5 feet, required the diameter of the pipe.

The constant logarithm is	—2·651500
Twice the log. of 12, (the quantity discharged) is.....	2·158362
The log. of the length (14637 feet) is.....	4·165541
	4·975403
The log of the fall (51·5 feet) is	1·711807
	5)3·263596
The log. of the diameter, or of 4·495 inches is	0·652719

The main pipe from Comiston to the Castle Hill reservoir of the Edinburgh water-service, delivered 11·9676 cubic feet per minute when the pipe was 4·5 inches in diameter. (Smeaton's Reports, vol. i. p. 231.)

To obtain a more accurate solution, repeat the operation, thus—

Constant logarithm	—2·651500
Twice the log. 12 (the quantity to be discharged).....	2·158362
The log. (14637 + 4·2 × 4·495) = log. 14656.....	4·166016
	4·975878
The log. 51·5	1·711807
	5)3·264071
The log. of 4·496, the diameter in inches	0·652814

This second operation may always be neglected when the length is so considerable; but in shorter lengths the correction becomes important. The allowance for bends in the pipes requires to be made, for which a simple method is given in the text.

Example 2.—The Swanston-pipe of the Edinburgh water-works was 6435 feet long, and had 168 feet fall, and delivered 4·55 cubic feet per minute: required its diameter.

Constant log.	—2·651500
Twice log. 4·55	1·316022
Logarithm of 6435.....	3·808549
	3·776071
Logarithm of 168	2·225309
	5)1·550762
Diameter 2·043 inches	0·310152

The actual diameter of the pipe was 2¼ inches, hence the bends and obstructions must have been considerable.

square multiplied by the sum of the sines of the several angles of inflection, and then by ·0038; which will give the degree of pressure employed in overcoming the resistance occasioned by the angles: and deducting this height from the height corresponding to the velocity, we may thence find the corrected velocity.

Mr. Eytelwein proceeds to investigate, both theoretically and experimentally, the discharge of water by compound pipes, with apertures of various dimensions between them; he allows at each orifice of the contraction of the stream, and calculates the height necessary to produce the increase of velocity in each instance, allowing also for the friction of the pipe. But the velocity thus found is somewhat smaller than the result of his experiments; probably because the whole of the force of the water accelerated at any orifice, is not immediately lost as soon as it arrives at a wider part of the pipe. The ascent of water in a compound pipe, to the level of a reservoir, is next considered, a case which often occurs in pump-work, and an approximation to the velocity of ascent is deduced from theory and compared with experiment.

CHAPTER X.

Of Jets of Water.

This chapter contains little that is new or interesting; it is well known that the velocity of a jet is greatest when it springs through an orifice in a thin plate, and in this case, the height falls little short of that of the reservoir.

CHAPTER XI.

Of the Impulse or Hydraulic Pressure of Water.

There are three principal cases of the impulse of water falling perpendicularly on plane surfaces: when a detached jet of water strikes the plane; when the plane moves in an unlimited extent of water, or is very small in respect to a stream that strikes it; and when the impulse takes place in a limited channel.

Supposing a stream of water to strike against a plane, so as to lose all its motion, it is obvious that the force that destroys the motion must be equal to the force that generates it; that is, to the weight of the column of water operating during the time necessary for its acquiring the given velocity; and the quantity of water arriving during this time, being equal to twice the column of which the length is the height due to the velocity, the hydraulic pressure must be twice the weight of such a column. The relative impulse against a plane in motion must be determined from the difference of the velocities; but when all the water of a stream strikes against a plane, the effect of the impulse may be more simply determined, as if a solid body struck the plane with the relative velocity: and this is nearly what happens in undershot water-wheels.

When a detached jet strikes against a plane, it appears from the experiments of Bossut and Langsdorf, that its effect is equal to the weight of an equal column of twice the height due to the velocity; but the plane must be at least four times as large in diameter as the jet; if it be only of the same size, the effect will be but one half as great. In an unlimited stream, the impulse is also nearly determined from the height corre-

sponding to the velocity; and it appears that the effect is nearly doubled by confining the stream to prevent its diverting laterally from the float-boards.

For oblique surfaces, the effect of a detached jet in its own direction appears to vary as the square of the sine of the angle of incidence; but, for motions in open water, we must add to this square about $\frac{2}{3}$ of the difference of the sine from the radius; a correction which is tolerably accurate, until the inclination becomes very great. Mr. Eytelwein found the resistance to the motion of a sphere nearly $\frac{4}{5}$ of the resistance to a circle equal to its section; perhaps it was a hemisphere, otherwise it is difficult to reconcile the result with other experiments in which it has appeared to be only $\frac{2}{3}$. Mr. Eytelwein informs us, that at the temperature 14° of Reaumur, or 63½ of Fahrenheit, a cubic foot of distilled water weighs 66·0656 pounds of Cologne, or 65·9368 commercial pounds of Berlin. According to Sir George Shuckburgh's experiment, an English cubic foot of distilled water of 66° weighs 997 ounces avoirdupois; and water expands for every degree ·000165: hence, the pound of Cologne is 1·0312 English avoirdupois pounds, and that of Berlin, 1·0332.

CHAPTER XII.

Of Overshot Water-Wheels.

The power which operates upon overshot wheels is divided into two parts, one derived from the weight of the water in the cells or buckets, the other from the impulse of the water falling on it. The effect of the first is constant; that of the second varies with the velocity: the *maximum* is found to be when the velocity is half that of the water received; but the variable part being the smaller, the rule is of little practical consequence,

and the velocity of the wheel is generally greater than this. The author observes, that, by turning the stream back upon the nearer half of the wheel, we remove the resistance of the lower water, since it runs off in the same direction with that of the water-wheel.*

CHAPTER XIII.

Of Undershot Water-wheels.

The author enters into a minute description of the parts of an undershot water-wheel: he observes, that the most advantageous position for the float-boards in a straight channel, is when they are perpendicular to the water at the time that they rise out of it; that only one-half of each should ever be below the surface; and that from three to five should be immersed at once, according to the magnitude of the wheel.† When there is sufficient fall, the float-boards should be divided and made into buckets, so that the wheel may become a breast-wheel; the position of the external portion being such, that a line drawn through it at the time when the water enters, may divide the vertical radius in the same proportion that it divides the quadrant of the circumference; that is, if the water is received, for instance, at one-third of the quadrant from the bottom, the line must be one-third of the radius above it. A formula is laid down for calculating the actual force of a given stream of water on a wheel; and it is shewn, that half the velocity of the stream is that which gives the maximum of effect, the theory agreeing perfectly with the experiments of Smeaton and

* See pp. 33–46, for the experiments of Smeaton and Bossut on this subject. —Ed.

† See pp. 2–32.—Ed.

others:* for since the effect is estimated by the product of the force into the velocity of the parts upon which it acts, and since the force is in this case simply as the relative velocity, because the quantity of water is given, and the whole of it is supposed, in all cases, to act; therefore, the effect will be expressed by the product of the relative and absolute velocity of the wheel, or $e = va$; but $r = v - a$; v being the velocity of the stream, and $ra = av - aa$, which is obviously greatest when $v = 2a$, as is evident either by taking the fluxion, or by considering that the greatest ordinate of a semicircle is the radius.

To shew the advantage of breast wheels over common undershot wheels, the author quotes Mr. Banks's experiments. He also observes, that, by placing two wheels after each other in the same stream, about one-fourth more force may be obtained than either by a single wheel, or by two wheels side by side; but that a single wheel has less friction, and is generally less expensive.†

CHAPTER XIV.

Of the Properties of the Air, as far as they relate to Hydraulic Machines.

What Mr. Eytelwein quotes as Mariotte's discovery of the increase of the air's density in proportion to the pressure, was well known to Hooke and Boyle. From the experiments of Woltmann and Schober, he remarks, that the force of the wind against a perpendicular plane, is nearly equal to four

* See pp. 27 and 29.—Ed.

† For modes of clearing mills of back-water and increasing the fall, see Venturi, Prop. ix. p. 163.—Ed.

thirds of the weight of a column of air, of a length equal to the height due to the velocity; the height of a column of water nearly equivalent to the force of resistance may be found, by taking the square of $\frac{1}{200}$ of the velocity in a second, in English feet.*

Thus, if the velocity were 1000 feet in a second, the resistance would be equal to a column of water on the same surface, 25 feet in height; and the resistance to a sphere about half as much.

For another example, if we had a cubic foot of a substance equal in specific gravity to water, and were desirous of knowing the greatest velocity that it could acquire by falling through the air; the height of the column of water is here 1, and its square root 1, which, multiplied by 200, gives 200 feet in a second for the velocity, when the resistance would be equal to the weight, which, of course, is the limit beyond which the velocity could never pass. Hence, we may form an idea of the utmost velocity that a stone, of moderate size, could acquire in descending from the upper regions of the atmosphere, or even from the neighbourhood of the moon, a velocity that would be much less than that of a bomb or a cannon ball even when it may be followed by the eye. Again, Mr. Garnerin's parachute contains about 860 square feet of surface, and weighs, together with the aeronaut suspended from it, about 230 pounds. Here the weight is $\frac{2}{8}\frac{3}{6}$ of a pound for each square foot, which is equivalent to $\frac{1}{230}$ of a foot of water; multiplying the square root by 200, we have about 13 feet in a second for the utmost velocity; which is the same as if one leaped from a height of between 2 and 3 feet. Mr. Garnerin, however, finds the mean velocity of descent only eight feet, which agrees better with the experiments of Borda, in which the resistance appeared to be $\frac{5}{3}$ of the weight of the column due to the velocity, and exceeded this proportion as the surface increased in magnitude.

* See p. 70.

CHAPTER XV.

Of Siphons.

For estimating the discharge of a siphon, the head of water must be reckoned equal to the difference between the levels of the surface of the water and of the lower orifice. The author observes that the theory of waves has been treated in a new and improved manner by Lagrange, in his Mécanique Analytique. The problem is, however, not yet completely solved: Lagrange's Formula includes the depth of the water agitated as a given quantity, but it does not inform us how to determine this depth from theory.*

CHAPTER XVI.

Of Sucking Pumps.

The length of the sucking pump must never be greater than 30 feet below the movable valve: and there may be a loss of time in the ascent of the water, unless it be made even a few feet shorter. The motions to be produced, and the resistances to be overcome, are considered in detail; but the author refers, for still further information, to Langsdorf's Treatise on Machinery.

* The theory of the tides, and the propagation of waves in the ocean, has lately been very fully investigated by Dr. Young, in Napier's Supp. to the Encyclo. Brit. art. "Tides."

The velocity of the stroke should never be less than four inches, nor greater than two or three feet in a second; the stroke should be as long as possible, to prevent loss of water by the frequent alterations of the valves; the diameter of the pipe should be about $\frac{2}{3}$ or $\frac{3}{4}$ of that of the barrel. The lifting pump is also here described; it only differs from the sucking pump in having the lower valve movable, and the upper one fixed. A number of valves and pistons are described in this chapter, chiefly from models of English manufacture.

CHAPTER XVII.

Of Forcing Pumps.

In describing the different kinds of solid piston, the author gives the preference to that which has a conical leather projecting on each side; but remarks that there is another form, which has the advantage in the situation of the ring for receiving the rod, which is precisely in the centre of the piston, and is, therefore, fitter for communicating motion in each direction. He says, that where the barrel is well polished, the piston may be used without either wadding or leather. The first pump, invented above a century before Christ, by Ctesibius of Alexandria, to whom, also, music is indebted for the organ, and whose name Mr. Eytelwein mentions in speaking of sucking pumps, was in reality a forcing pump, as may easily be collected from its description by Vitruvius (L. x. chap. 12.)

CHAPTER XVIII.

Of mixed Pumps, or the Combination of Sucking and Forcing Pumps.

When the lower valve is above the surface of the water, the forcing pump can only raise the water by suction, but the construction remains the same; such is Mr. Buchanan's patent ship pump. De la Hire's pump is more complicated; both the ascending and descending strokes of the piston being made effective, by means of a double apparatus of valves and pipes.

CHAPTER XIX.

Of acting Columns of Water.

The mechanism of a pump may be employed for converting the weight of water descending in its barrel to the purpose of working another pump. The author describes a machine of this kind, invented by Mr. Höll, and improved by Langsdorf. A similar arrangement, used in Cornwall, has lately been described in Nicholson's Journal, by Mr. Trevithick. The only objection to it appears to be the magnitude of the friction.

CHAPTER XX.

Of the Spiral Pump.

If we wind a pipe round a cylinder, of which the axis is horizontal, and connect one end with a vertical tube, while the other is at liberty to turn round and receive water and air in each revolution, the machine is called a spiral pump; it was invented, about 1746, by Andrew Wirz, a pewterer in Zurich, and was employed at Florence with Bernoulli's improvement, in 1779. At Archangelsky, near Moscow, a pump of this kind was erected in 1784, which raised a hogshead of water in a minute to a height of 74 feet, and through a pipe 760 feet in length. The force employed is not mentioned; we may, therefore, conjecture that it was turned by water. Mr. Eytelwein enters very minutely into calculations of the effect of such a machine under different circumstances; and the results of the theory, as well as of experiment, are such as to induce us to expect that it will in time come into common use, instead of forcing pumps of a more complicated and expensive construction. The water-tight joint presents the only difficulty; the pipe may form either a cylindrical, a conical, or a plain spiral, and it appears to be uncertain which is the most advantageous; the vertical pipe should be nearly of the same dimensions as the spiral pipe, which may without difficulty be made of wood.

CHAPTER XXI.

Of the Screw of Archimedes, or the Water-snail, and of the Water-screw.

The screw of Archimedes consists, either of a pipe wound spirally round a cylinder, or of one or more spiral excavations, formed by means of spiral projections from an internal cylinder, covered by an external coating, so as to be water-tight. But if the coating is detached, so as to remain at rest while the spirals revolve, the machine is called a water-screw. Mr. Eytelwein enters into an accurate determination of the effects of these machines in given circumstances; and the results of the theory agree remarkably well with experiment. He observes, that the screw of Archimedes should always be so placed, as to fill exactly one half of a convolution in each turn; and that very unfavourable reports have sometimes been made of the machine for want of attention to this circumstance, for, when the orifice remains constantly immersed, the effect is very much diminished: this appears also to have happened in some late experiments in London. When the height of the water is so variable as to render this precaution impossible, Mr. Eytelwein prefers the water-screw; although, in this instrument, one-third of the water runs back, and it is easily clogged by accidental impurities. The screw of Archimedes is generally placed so as to form an angle of between 45° and 60° with the horizon, but the open water-screw at an angle of 30° only: for great heights, the spiral pump is preferable to either.

CHAPTER XXII.

Of Bucket-Wheels and Throwing-Wheels.

In the construction of wheels for raising water in buckets, there is little room for refined theory, whether the buckets be fixed or suspended on an axis. It is sometimes convenient to raise water to the height of 3 or 4 feet by the revolution of a wheel with simple float-boards; and such a wheel may be either in a vertical or an inclined position, it must, of course, be inclosed in a sweep.

CHAPTER XXIII.

Of Cellular Pumps and Paternoster-works.

Water has been sometimes raised by stuffed cushions connected with an endless rope, and caused by means of two wheels or drums to rise in succession in the same barrel, and to carry water with them; but the magnitude of the friction appears to be an objection. From the resemblance of the apparatus to a string of beads, it has been called a Paternoster-work. When flat boards are united by chains, and employed instead of these cushions, the machine may, without impropriety, be called a cellular pump: here the barrel is generally square, and placed in an inclined position. But these machines are very rarely employed.

CHAPTER XXIV.

Of Instruments for Measuring the Velocity of Streams of Water.

The superficial velocity of a stream is ascertained without difficulty, by observing, in calm weather, the motion of a body barely floating on it. But it is more difficult to determine the velocity of a river at a considerable depth. Pitot's tube, as improved by Buat, furnishes one of the easiest methods. A funnel is presented to the stream, and the water, in a vertical tube connected with it, is elevated above the level of the river, nearly to the height corresponding to the velocity. But the result will be more accurate, if the funnel be covered with a plate perforated only in the centre by a small orifice; in this case, the elevation in the tube will be half as great again as the height due to the velocity. Other instruments for appreciating the impulse of the water against a flat board, require some previous comparative observation. In Woltmann's hydrometrical fly, the number of revolutions of a wheel, in a given time, indicates the velocity of the water, which strikes against two inclined planes, and carries round the arm to which they are fixed.

It is presumed that this abridged account of Mr. Eytelwein's book, will not only do justice to his diligence and ingenuity, but will convey to the English reader some matter perfectly new, and capable of frequent application in practical hydraulics; which is, perhaps, of more value, as there is little probability that the work will be translated at length. To disseminate information of this kind must always be the principal object of the Journals of the Royal Institution.

THE END.

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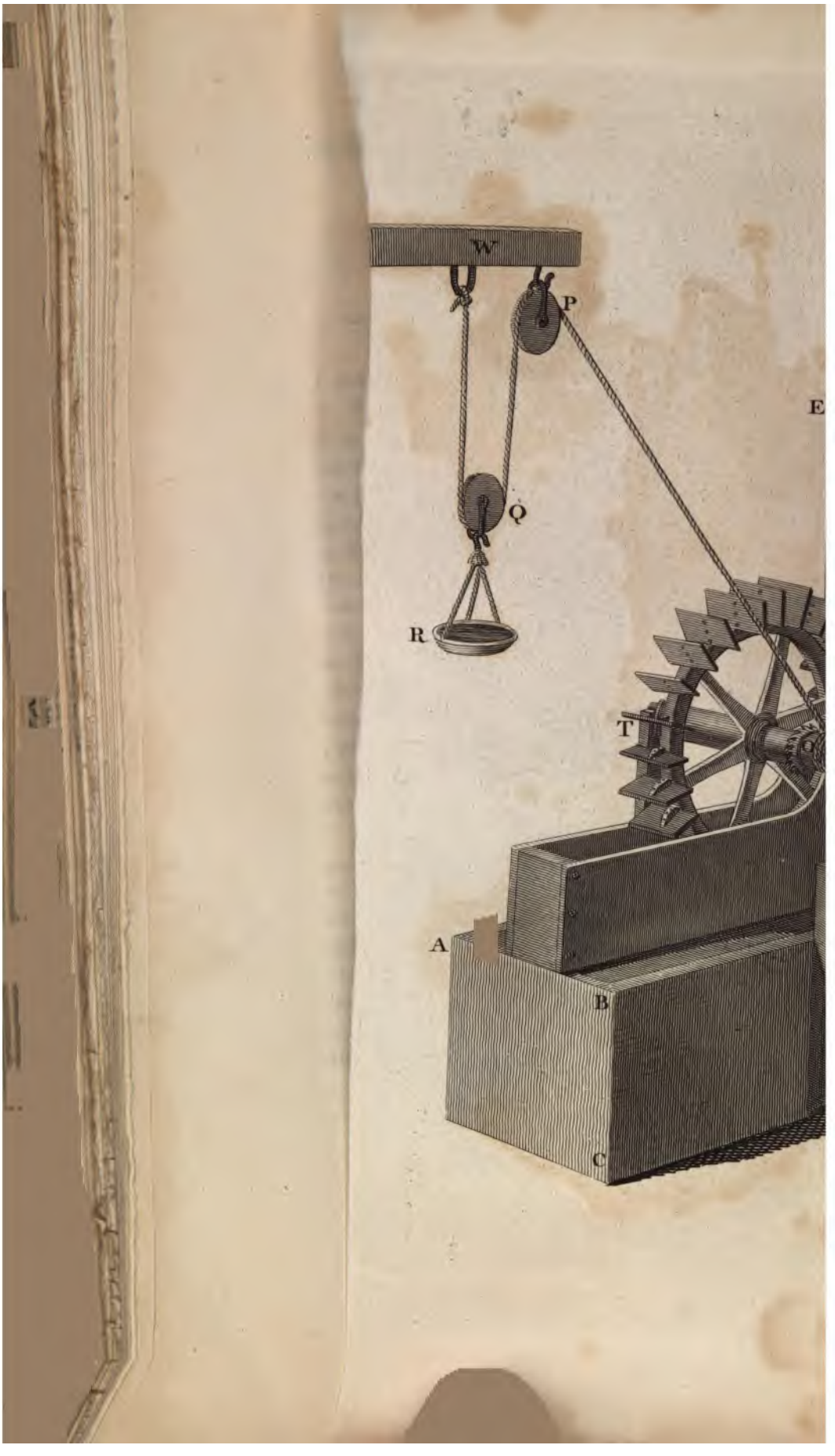
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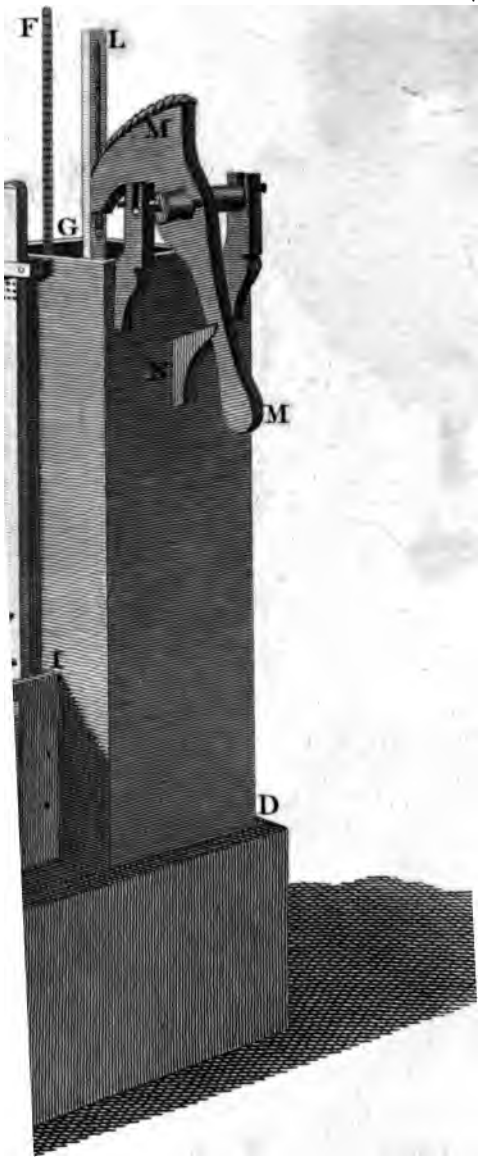
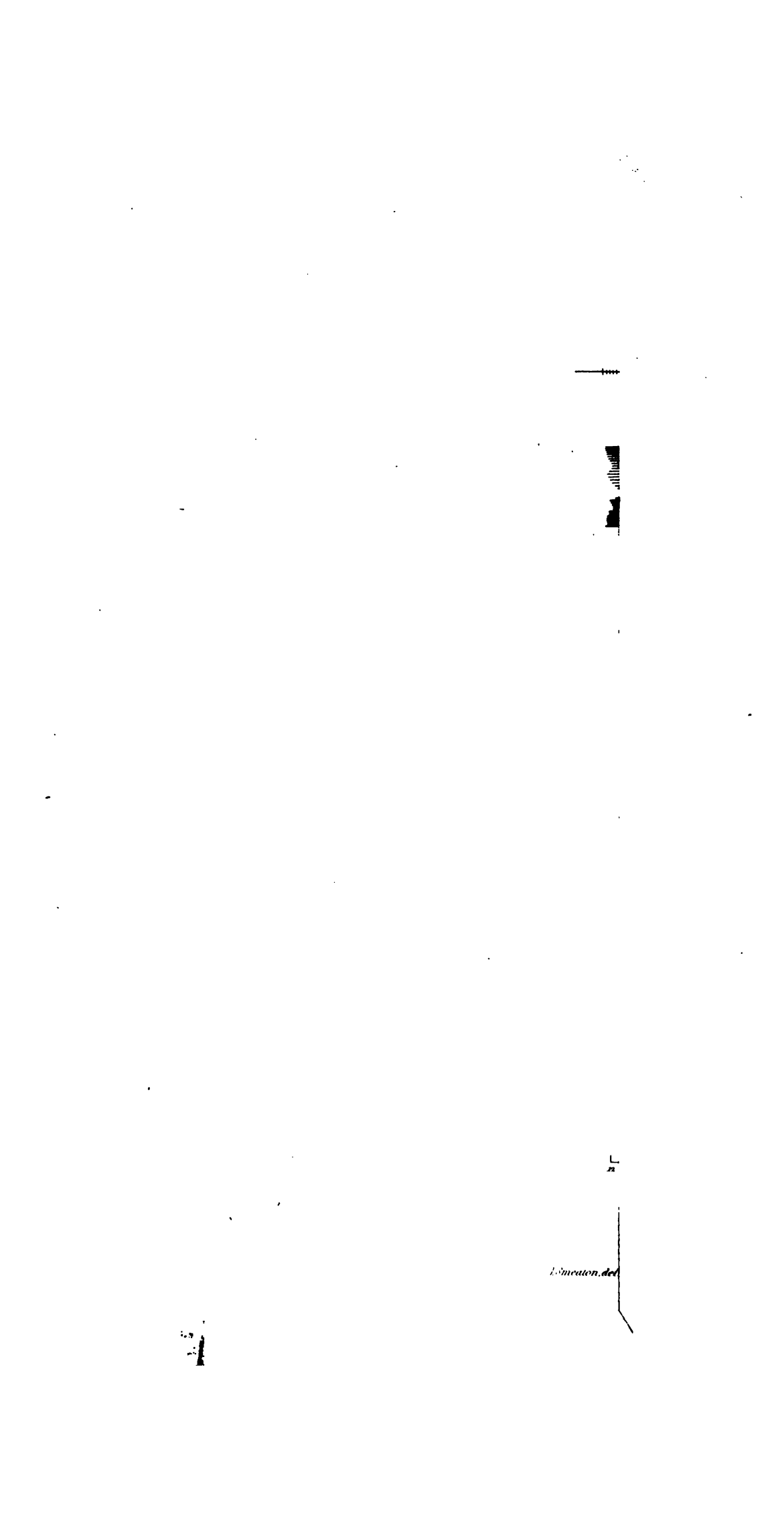
Smeaton's Experiments. Plate I.





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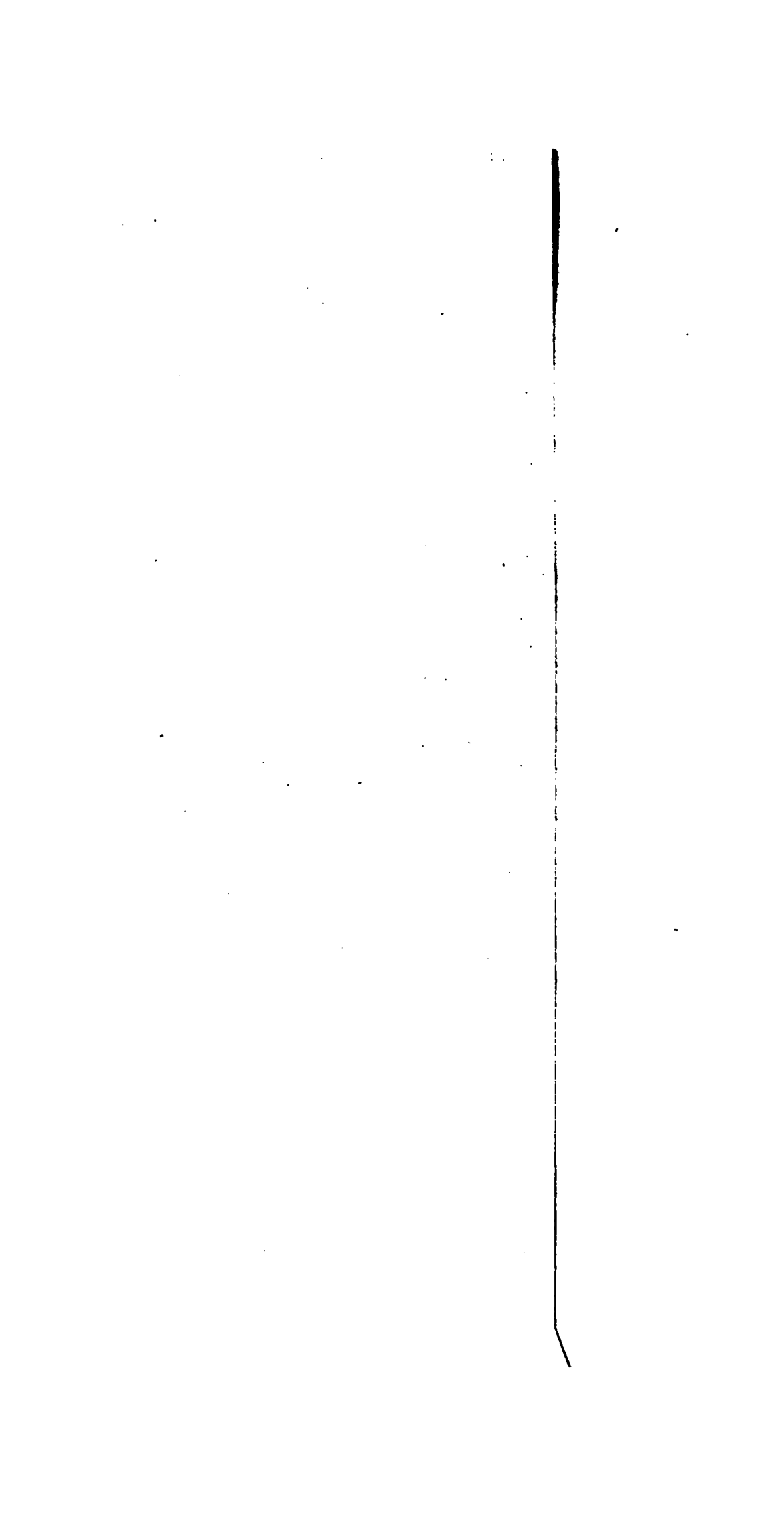

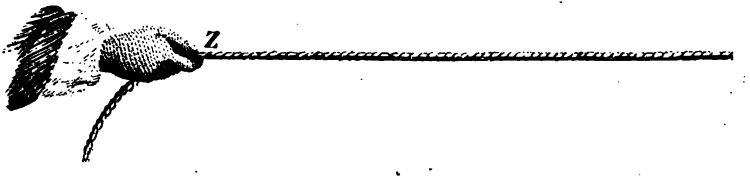

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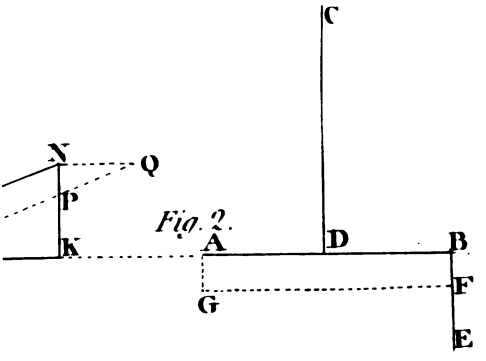

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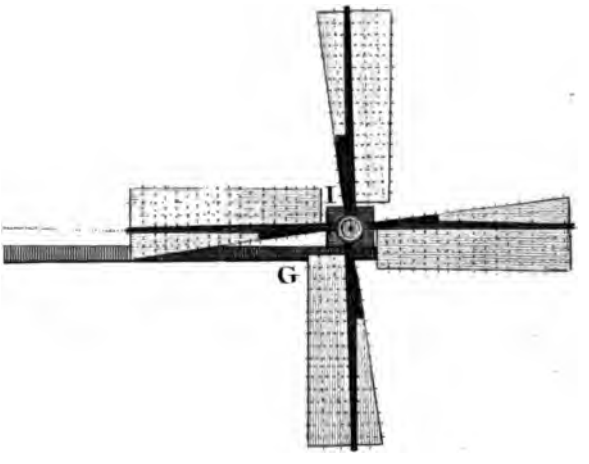

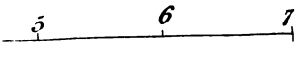

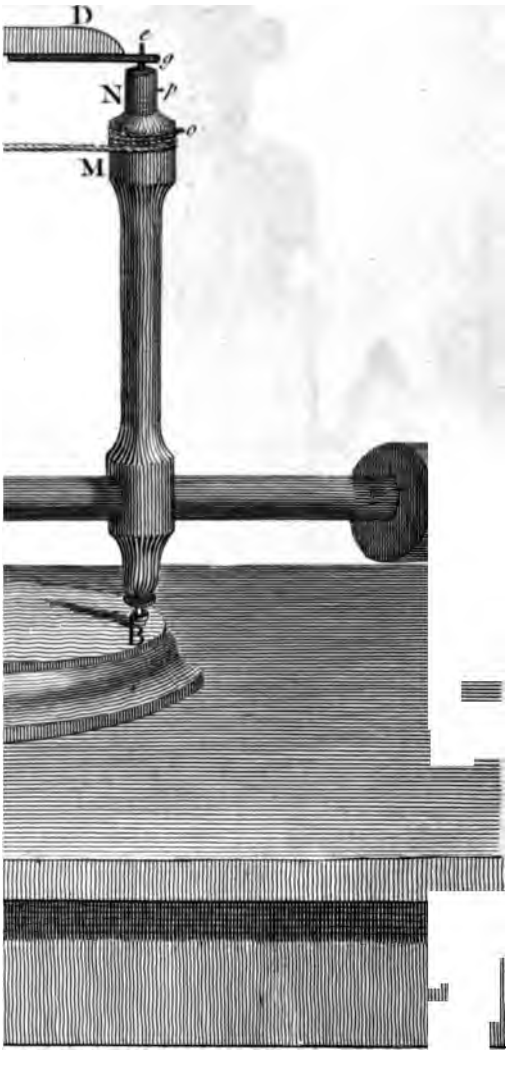
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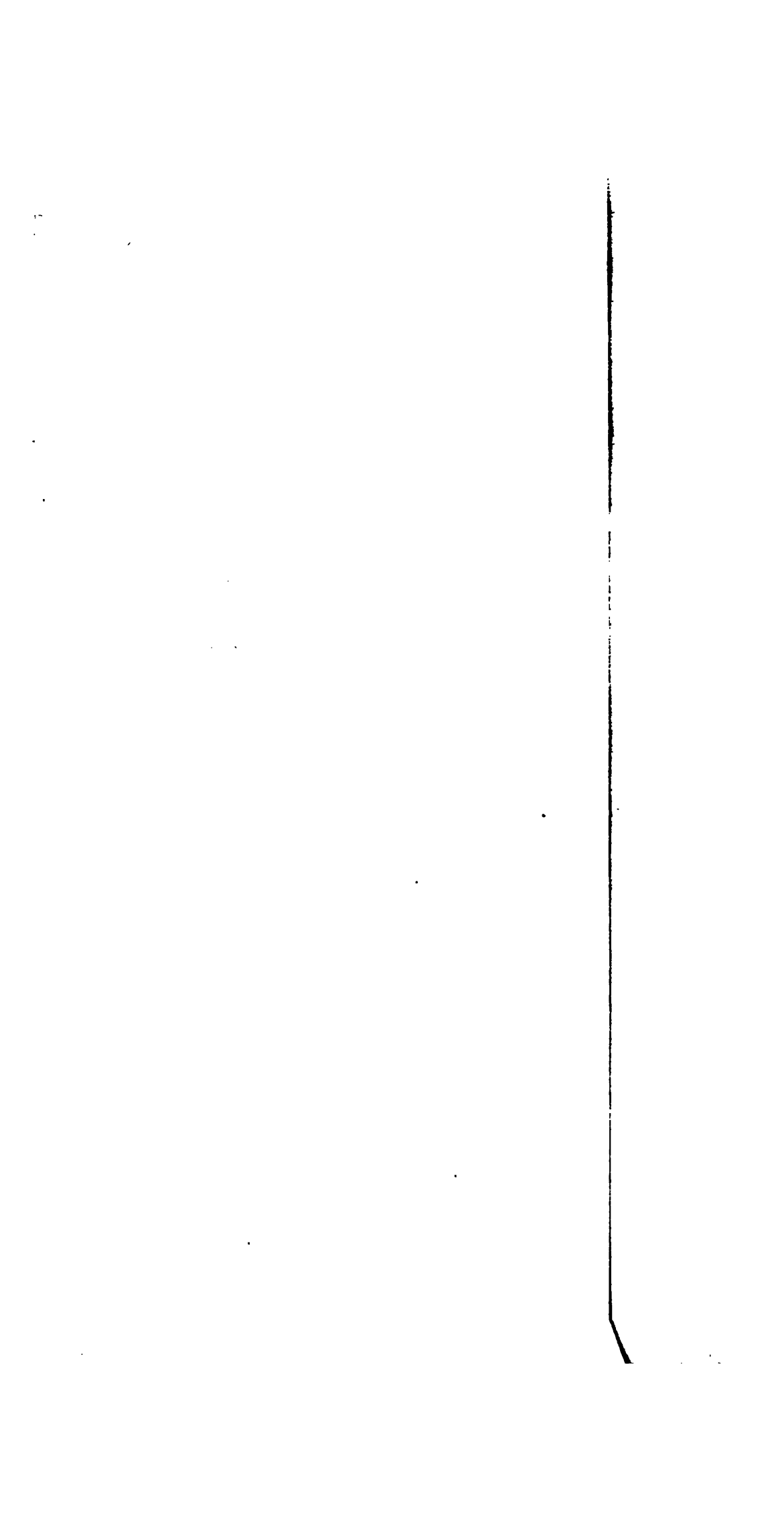

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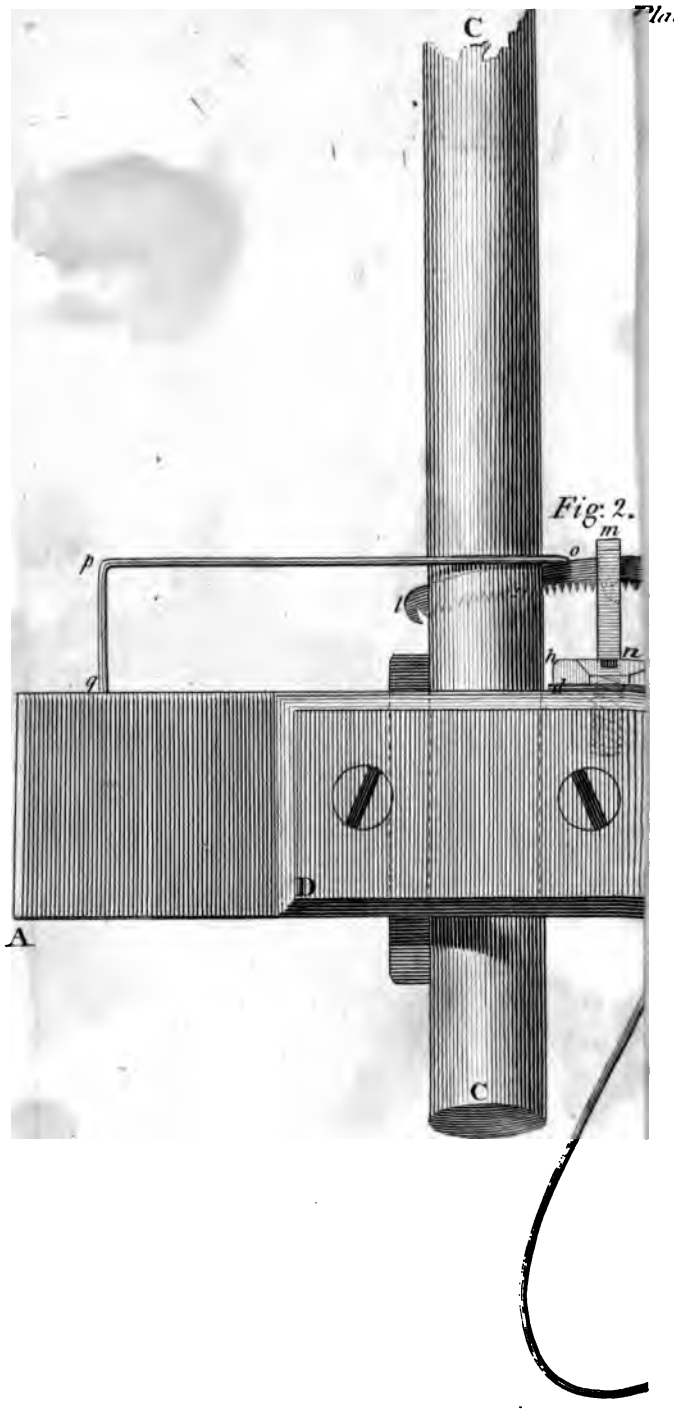

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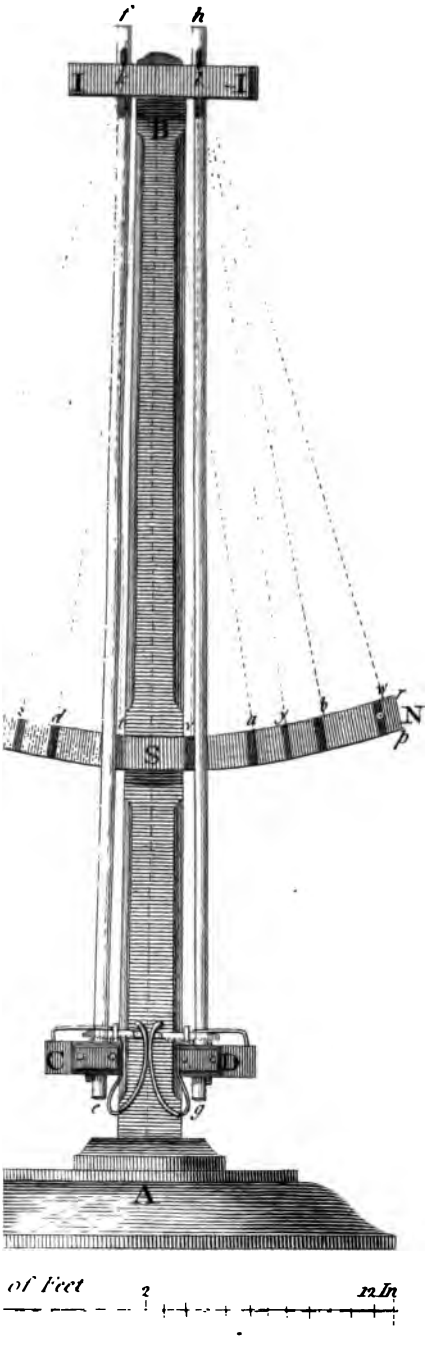
Smeaton's Experiments Plate V.



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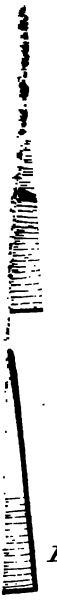

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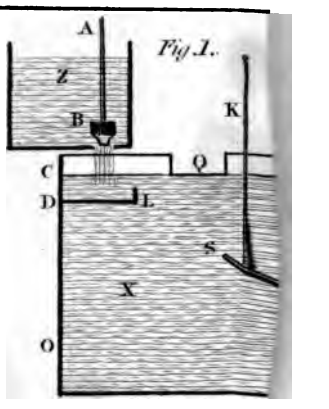

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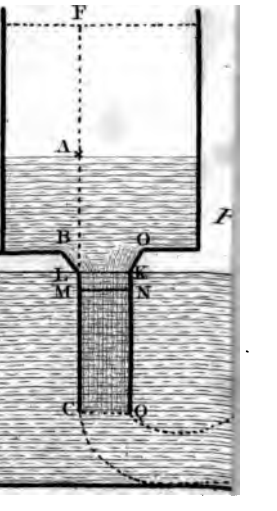

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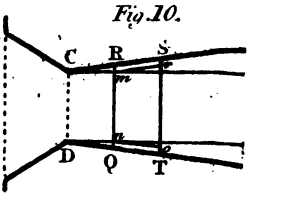

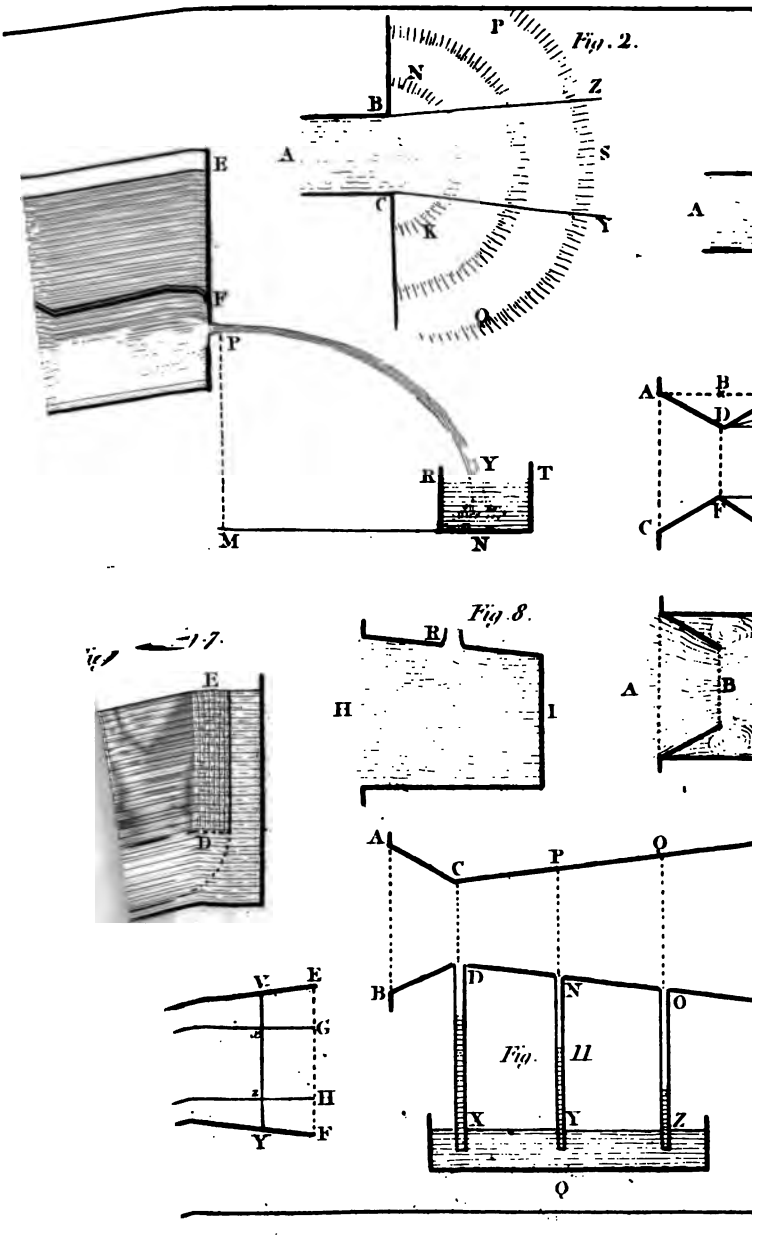
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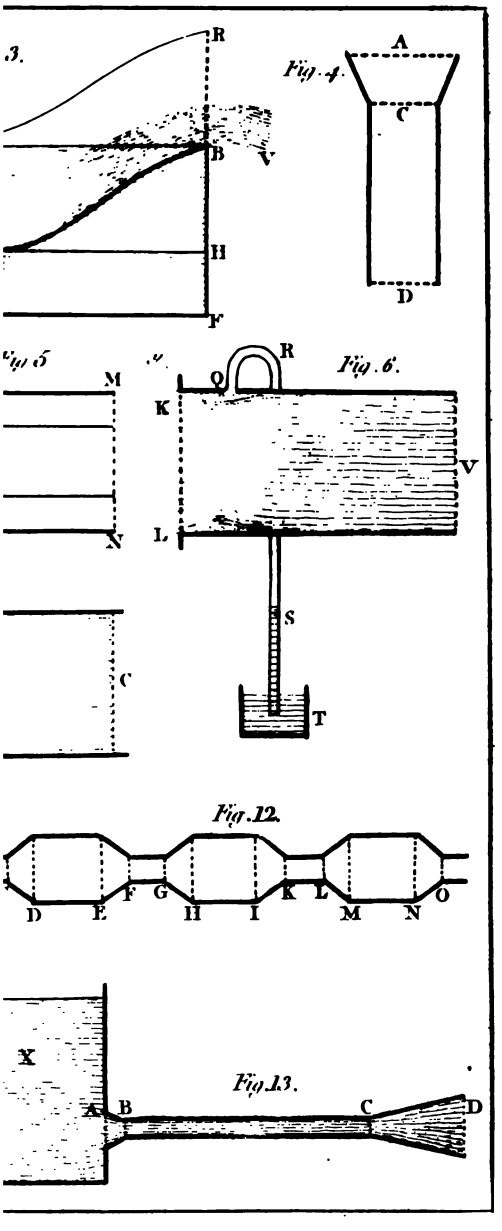
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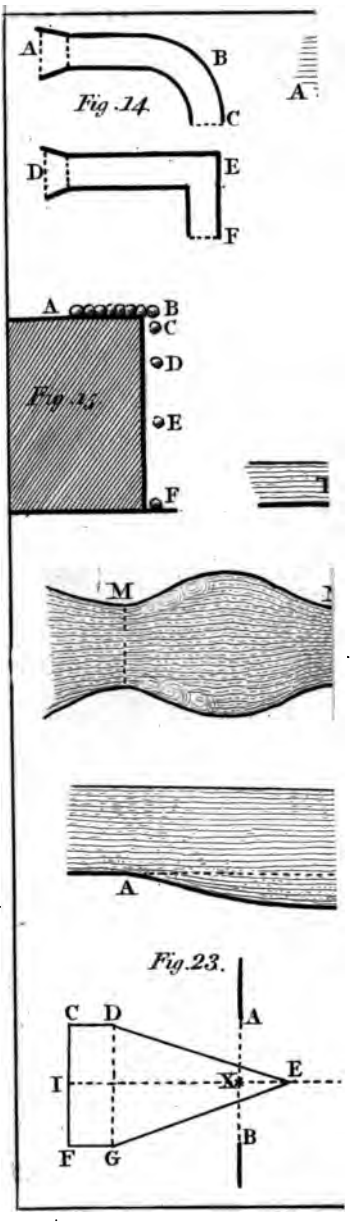
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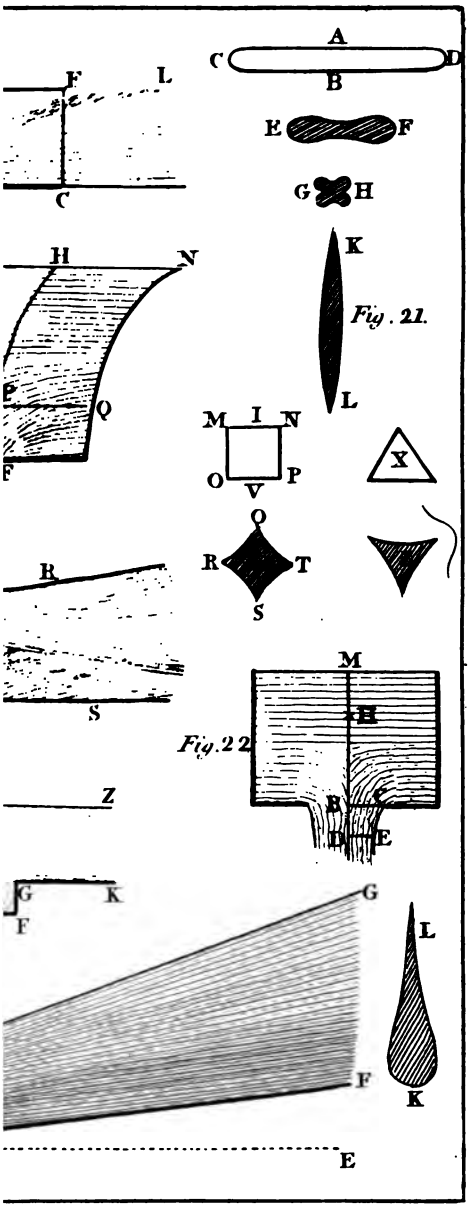
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